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Lectures
ON
THE SYMPATHETIC INNERVATION OF STRIATED MUSCLE.

BY
THE LATE JOHN IRVINE HUNTER, M.D., CH.M.,
CHALLIS PROFESSOR OF ANATOMY IN THE UNIVERSITY OF SYDNEY.

EXPLANATORY INTRODUCTION

BY
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[When Professor Hunter arrived in England last November he was invited by the University of London to give three advanced lectures in anatomy, which were to have been delivered in the second week of December: but the large audience that assembled to hear the first lecture were informed of the grave illness to which he succumbed two days later (see BRITISH MEDICAL JOURNAL, December 20th, 1924).

He had been looking forward with the keenest interest to the delivery of these lectures, because they would have provided him the opportunity for clearing away the misunderstanding of his work and for demonstrating the adequacy and reliability of his evidence and the inferences he had drawn from it. In particular he was anxious to explain the scientific principles that should guide the surgeon in the selection of patients for the operation that he had devised (in collaboration with Dr. Norman Royle) for the relief of certain patients subject to spastic paraplegia—namely, those whose voluntary control of muscular actions was unimpaired except for the interference resulting from an exaggeration of what Sherrington calls "plastic tone"; and it had been his intention to describe the surgical procedure in so far as the reasons for the choice of the rami to be cut were concerned.

In the third lecture he proposed to discuss the discordant conclusions that had been drawn by different physiologists and surgeons respectively from their experiments (especially in the cat) and operations on the sympathetic in human patients, and to give the explanation of this apparent lack of consistency.

Finally, he intended to deal with the wider scientific bearings of his anatomical and experimental researches, and the possibilities thus opened up for promising investigations in many directions—anatomical, physiological, pharmacological, and clinical.

This much the compiler of these reports gathered in conversation with Hunter, who, unfortunately, had not prepared any manuscript of the three lectures he had intended to give. It had been his intention, as was his usual custom, to speak without notes, and apart from these conversations and the large series of lantern slides and cinematographic films, he has left nothing in the form of lectures. Yet the subject is so important in its scientific bearings, and the need for some explicit guide to surgeons is so urgent, that the attempt has been made to do what Hunter had intended to do, and to publish the reports.

The compiler has done nothing more than select passages from Hunter's published and unpublished works and endeavour to give a connected and consistent account of his results. In addition to the three memoirs published in the *Medical Journal of Australia* (January 26th, June 14th, and September 27th, 1924), in *Brain* (August, 1924), and in *Surgery, Gynecology and Obstetrics* (December, 1924), the compiler has in his hands the manuscripts of three unpublished memoirs of Hunter's on this subject, as well as the shorthand reports of discussions in which he took part at Brooklyn and Philadelphia in October. Moreover, Hunter had intended to make use of the information provided by Orbeli and his collaborators, of which an account was given by Dr. W. Horsley Gantt in the BRITISH MEDICAL JOURNAL of September 20th, 1924 (p. 533).

* By whom the lectures were delivered at University College, London, January-February, 1925.

While the statements made in the following pages are Hunter's, any faults of arrangement or of literary form are to be attributed to the compiler. Discussing the problem which Hunter sets out to solve in these lectures Pekelharing[1] twelve years ago wrote: "The most direct investigation of the mechanism of muscular contraction will be afforded by microscopic study: but, unfortunately, we must wait for a happy inspiration to reveal the road to follow." The compiler of these lectures is convinced that Hunter has got the "happy inspiration."]

LECTURE I.
THE DUAL INNERVATION OF STRIATED MUSCLE.

IN his classical memoir on postural activity of muscle and nerve Sir Charles Sherrington[2] makes the statement that: "The existence in various invertebrata of muscles separately differentiated for execution of movements and for maintenance of posture respectively seems without parallel in the skeletal musculature of vertebrates. In the latter, one and the same muscle is used for the two purposes, though some muscles are predominantly concerned with the one, some with the other function." Discussing the same subject, the late Sir William Bayliss was frankly sceptical. Thus, in his *Principles of General Physiology*[3] he says: "It is not easy to understand how two fibres of different function and different innervation could coalesce with retention by the combined cell of both kinds of innervation." The chief aim of these lectures is to justify Bayliss's attitude of scepticism by showing that no such coalescence does in fact occur, for the muscular tissues of vertebrates are at least as highly differentiated in structure and function as those of invertebrates.

In his discussion of the sympathetic innervation of striated muscle that led to this expression, Bayliss admits that "two kinds of function are performed by two separate kinds of muscle fibres, as in the auricle of the tortoise, or by separate muscles, as in the mollusc"; yet he goes on to express the commonly accepted view in these terms: "But in other cases, as in the vertebrate bladder or in that of voluntary muscles, the same fibres undertake both functions, so far as can be made out." I shall first give reasons for the conclusion that the same fibres do *not* undertake both functions.

The evidence in support of this conclusion is provided (*a*) by the appreciable structural differences in the two kinds of fibres in striated muscles; (*b*) by different modes of innervation of the thick and the thin fibres; (*c*) by the totally different effects produced by cutting the medullated (somatic) and the non-medullated (sympathetic) nerves passing respectively to the thick and the thin fibres; and (*d*) the interpretation of the effects of stimulation of the sympathetic or the influence of adrenaline, veratrine, etc., such as Oliver and Schafer (1895), Cannon and Nice (1913), Cannon and Cattell (1916), and Orbeli and his collaborators (1922-24), amongst others, have recorded.

THE ANATOMICAL EVIDENCE OF TWO DISTINCT KINDS OF STRIATED MUSCLE FIBRES.

Forty years ago Ranvier called attention to the differences in structure in red and white muscles, and since then he and many others have examined the evidence more fully and correlated the anatomical facts with differences in function of the two types of muscle. In his *Textbook of Microscopic Anatomy*[4] Sir Edward Sharpey-Schafer has summed up the evidence so clearly that I cannot do better than quote his statement:

"In the rabbit, as pointed out by Ranvier (1887 and 1889) and Krause, certain of the voluntary muscles present differences in appearance and mode of action from the rest. Thus, while most of the voluntary muscles have a pale aspect and contract energetically when stimulated, some . . . are at once distinguished by their deep red colour, as well as by their slow and prolonged contraction when stimulated. When subjected to microscopical examination it is found that in the red muscles the fibres are more distinctly striated longitudinally and the transverse striae are more irregular than usual. The muscular fibres are generally finer (thinner) than those of the ordinary muscles, and have a larger amount of sarcoplasm. The nuclei are more numerous and are not confined to the inner surface of the sarcolemma, but occur scattered in the thickness of the fibre as well. There is also a difference in the blood supply of the two kinds of muscle. . . .

"A similar difference between red and pale muscles may be also seen in the rays amongst fishes. In other animals the distinction is not found as regards whole muscles, although it may affect individual fibres of a muscle. This is the case, as shown by Klein, in the diaphragm, in which in many of the fibres there are numerous nuclei, and these are imbedded in protoplasm (sarcoplasm), which forms an almost continuous layer underneath the sarcolemma. The distribution of the two kinds of fibres in different muscles has been especially investigated by Gruetzner (1884)."

The contrast between the structure, mode of innervation, and functions of the two types of muscle, as well as the striking differences in their respective liability to fatigue, have been emphasized by many other investigators; and I might mention the memoirs of Dr. John Hay[5] and Dr. Ffrangcon Roberts[6] as important contributions to the evidence and interpretation of this interesting fact.

These records establish quite definitely the fact that there are two kinds of striated muscle, distinguished by contrasted morphological and physiological properties. But it is important not to confuse red and white muscles with the thin and the thick fibres respectively. While it is established that the sluggish red muscle is composed mainly of slender fibres innervated mainly by fine nerve fibres, and the briskly reacting and easily fatigued white muscle of thick fibres innervated by large medullated fibres, it yet remains to be decided whether or not all striated muscles are composed of both kinds of fibres—obviously, of course, in very different proportions in the various muscles.

It is right that some reference should be made here to Gruetzner's insight when, in 1887, he put forward his brilliant suggestion as to the real meaning of the work of Ranvier and his collaborators. Gruetzner attempted to correlate the physiological phenomena exhibited by skeletal muscle with the then recent developments in knowledge of the histology of muscle. He developed the idea of an "intrinsic support" within the muscle, enabling one contraction to be superposed upon another. He put forward the view that some muscle fibres were responsible for the contractions and others for the "intrinsic support." If some muscle fibres are supplied by somatic nerves and the remainder by sympathetic nerves only, the general principle of the functional duality theory of Gruetzner would, in the light of the experimental analysis undertaken by Royle and me, be established.

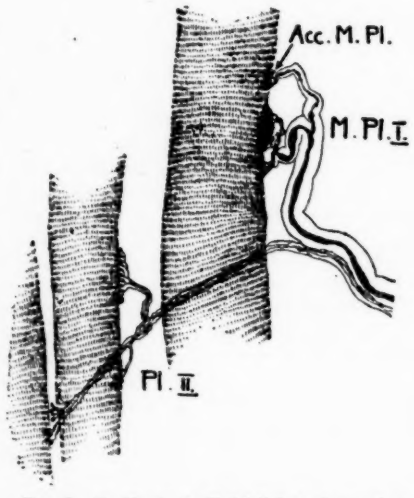

FIG. 1.—To illustrate Kulchitsky's demonstration of the dual constitution of striated muscle—large fibres innervated by medullated nerves ending in muscle plates (M. Pl. I and Acc. M. Pl.) and slender fibres innervated by non-medullated (sympathetic) nerves with so-called "grape-like" terminations (Pl. II).

DIFFERENCES IN THE INNERVATION OF THE TWO KINDS OF STRIATED MUSCLE FIBRE.

It was Kulchitsky's demonstration[7] of the fact that "the medullated and non-medullated fibres never terminate in the same muscle fibre" that impelled me to investigate by experiment the influence of the non-medullated nerves, which Boeke had clearly proved to be sympathetic, upon muscular function. During the year 1922 I was fortunate in being able to work with Professor Kulchitsky at University College, London, and to be permitted to take back to Sydney (early in 1923) some of his beautiful preparations of the endings of the medullated and non-medullated nerves in striated muscle, the description of which he did not publish until a year later. When I returned to Australia Dr. Norman Royle (with whom I had previously collaborated in an experimental investigation of the effects of transverse lesions of the goat's spinal cord) consulted me with reference to the inadequacy of the surgical operations to relieve spastic paraplegia. We studied Kulchitsky's preparations of nerve endings and the review of the present state of our knowledge by our former master, Professor J. T. Wilson,[8] and decided that, as certain of the muscle fibres were innervated solely by the sympathetic, some definite effect ought to be produced by cutting the sympathetic nerves going to limb muscles.

Hence we devised the series of experiments, and eventually the surgical operations which I shall discuss in the second and third lectures. But before doing this it is essential that I should consider the problem of the nerve endings, which is a matter of fundamental importance in this inquiry.

It has already been mentioned that as early as 1884 Ranvier called attention to the fact that there are two kinds of striated muscle fibres—one slender and rich in sarcoplasm with irregular transverse striae, the other thicker and much more energetic. Five years earlier (1879) Tchiriev had recorded that the thinner fibres are innervated by non-medullated nerves with grape-like endings, whereas the thick fibres are supplied by medullated nerves with end-plates such as Kuhne had described. This important discovery attracted little attention because Tchiriev supposed the fine muscle fibres with their non-medullated nerves were merely embryonic forms of the larger fibres with medullated nerves.

In 1902 Perroncito confirmed Tchiriev's observations that the different kinds of nerves (medullated and non-medullated respectively) were not supplied to the same muscle fibre. But since then Boeke has put forward the claim that both kinds of nerve may end in the same muscle fibre; and for more than ten years experimenters and morphologists have been trying to harmonize the facts of their fields of investigation with Boeke's statement.

Before giving the reasons for dissenting from these views, which are opposed to the observations recorded by Tchiriev, Perroncito, Kulchitsky, and others, reference must be made to Boeke's important demonstration of the fact that the non-medullated nerve fibres proceeding to striated muscle are really sympathetic. By the application of the experimental method to the elucidation of morphological problems he was able to show that the non-medullated nerves in question are the terminal portions of grey rami communicantes which arise in the thoracico-lumbar ganglia of the sympathetic cord. Boeke found in cats that three weeks after section of the nerves to certain of the eye muscles near their origin from the brain stem, many non-medullated fibres had resisted degeneration. He rightly concluded that these were sympathetic fibres from the cervical sympathetic chain which had been transferred to the eye muscle nerves distal to the point of experimental section of these nerves. Boeke performed the converse experiment of removing the superior cervical ganglion, but, beyond the fact that he was able to record the impression that the non-medullated fibres were fewer in number at the end of two weeks, this experiment was unsuccessful. Agduhr, on the other hand, after removal of the ganglion stellatum in a cat, was able to find, on killing the animal six days later, the remains of degenerated non-medullated nerves supplying skeletal muscles of the fore limb. It must be accepted, therefore, that some at least of the non-medullated fibres found supplying skeletal muscle take their origin from the vertebral ganglia of the ganglionated sympathetic chain. To employ the terminology of Langley, who confines the term "sympathetic" to the thoracico-lumbar outflow, the skeletal muscles receive post-ganglionic non-medullated fibres of the sympathetic nervous system by way of the grey rami communicantes. In addition, of course, this muscle receives the ordinary cerebro-spinal nerve fibres of somatic origin. In this sense skeletal muscle has a double innervation.

But if Boeke and Agduhr have rendered this great service of establishing the origin of the sympathetic fibres to striated muscle—a discovery which necessitates a new orientation of our ideas of the sympathetic and stultifies the definition of the sympathetic system still found in all

our textbooks—they have also introduced an element of discord by their claim that both kinds of nerve end in the same muscle fibre.

Limitations of space make it impossible to give an adequate account of my reasons for rejecting the opinion of Boeke on this matter. In collaboration with my colleague, Dr. Oliver Latham, I have carefully examined the mode of ending of the nerves proceeding to the limb muscles of the goat and fowl, the animals used by Dr. Royle and myself in our experimental work. A full account of our results will soon be published; all I need say at present is that in no case were we able to discover a muscle fibre receiving both kinds of nerve fibres. In this respect our results confirm those previously obtained by Kulchitsky and the pioneers Tchiriev and Perroncito. Kulchitsky is not willing to admit that any of the grape-like endings of the non-medullated nerves are hypolemmal, whereas Tchiriev says they are definitely hypolemmal. Latham was able to show that there are two kinds of nerve endings on the slender muscle fibres—those of the efferent nerves, which are the grape-like hypolemmal terminations, and others, presumably afferent epilemmal endings. It is interesting to note, in confirmation of these claims, that Agduhr states that he has Bielschowsky preparations to demonstrate that, in addition to hypolemmal terminations, "there are also epilemmally situated sympathetic terminal plates in the limb musculature." It is evident, therefore, that efferent (hypolemmal) and afferent (epilemmal) sympathetic nerve endings are distinguishable from one another. In the next lecture I shall demonstrate the experimental proof of the existence of both afferent and efferent fibres from the sympathetically innerved muscle fibres.

In collaboration with Mr. R. E. Murray, B.Sc., of the University of Sydney, I have made a critical examination of the evidence cited by Boeke and Agduhr in support of their claim for the double innervation of individual muscle fibres. Our memoir on this important issue is ready for publication with a series of illustrations. At present I shall quote merely certain passages from the full statement.

Boeke established the existence of a plexus of non-medullated nerve fibres supplying skeletal muscle, which appeared to be quite independent of the ordinary medullated nerve fibres. He termed this system of fibres together with their terminations on striated muscle fibres "accessory," and in 1913 suggested that this system was sympathetic or autonomic in character. He supplemented his morphological researches by experimental investigations, which he summarized in 1917 in an extensive memoir. See also his London lecture published in *Brain* (1921). This work proves beyond doubt the validity of his contention that the extrinsic striated muscles of the eyeball are innervated by sympathetic as well as by the cerebral motor nerves. But at the same time his results raise a doubt whether he is right in describing all the fibres and endings which he originally termed "accessory" as being sympathetic in origin.

The most striking result which points to the improbability of Boeke's contention is provided by the fact that a marked difference in the histological picture of the nerve supply to the extrinsic muscles of the eye is to be seen after section of the cerebral motor nerves to these muscles, according to the time after the operation at which the examination of the muscle is made. In some cases he allowed a short period for degeneration (three to five days), and in others a longer period—for example, three weeks. After a short period specimens stained by the Bielschowsky method revealed that all the medullated fibres and their associated end-organs were undergoing degeneration. But the non-medullated "accessory" system was so well represented that apparently at least one "accessory" nerve ending was to be seen on each muscle fibre. In similar preparations of material in which degeneration had proceeded for the longer period, however, the greater number of these non-medullated fibres had disappeared. The conclusion justified by these observations is that the minority of the non-medullated nerve fibres which resist degeneration altogether are derived from sympathetic communications to the motor cerebral nerve which join the nerve distal to the site of section. On the other hand, the degenerated fibres and terminations of the "accessory" system of Boeke obviously belong to another category. Boeke's explanation of the failure of these fibres to resist degeneration is that they are cranial autonomic in origin; but, as Wilson[9] points out, he does not indicate in which ganglion the cells providing these non-medullated fibres are situated. Wilson also calls attention to the fact that, in any case, the section of the motor cerebral nerve is close to the brain stem, and the ganglion should remain uninjured, and therefore degeneration should not occur. Furthermore, Wilson raises the point whether Boeke has established beyond doubt that the tardily degenerating fibres are actually non-medullated rather than finely medullated fibres whose sheaths have already suffered degeneration. Nor does Boeke's converse operation of removing the superior cervical sympathetic ganglion establish that all the "accessory" fibres are sympathetic in origin. For the entire system did not disappear, though Boeke thought the fibres were fewer in number.

These anomalies in Boeke's interpretation of the origin of the "accessory" system undermine his contention that every individual muscle fibre receives both a sympathetic and somatic nerve ending, for any given case cited by Boeke may well be an example of two somatic endings, and not of double innervation in the sense in which that term is used in this lecture.

In later experiments by Boeke and Dusser de Barenne (upon the double innervation of the intercostal muscles of the cat) no mention whatsoever is made of the relation of the somatic and sympathetic endings to each individual muscle fibre. This work proves only that the intercostal muscles as a whole receive both sets of nerves.

In this respect the evidence of Agduhr is of great importance. This author investigated experimentally the result of partial denervation (somatic or sympathetic) of the muscles of the fore limb of the cat. After removal of the ganglion stellatum he succeeded in observing in Bielschowsky preparations of the brachial musculature, secured six days after the operation, the remains of degenerated non-medullated nerves. He also cut the last four cervical and the upper two thoracic nerves in the intervertebral foramina between the spinal ganglia and the point of divergence of the white ramus communicans. He killed the animal, the histological investigation of which is described in detail, five days after the operation; but he records finding similar appearances after a period of ten days. He observed that all the medullated fibres (motor and sensory) had undergone degeneration. On the other hand, many non-medullated fibres were seen to be intact; they were lying along blood vessels and were partly bound up with degenerated medullated fibres.

The point of interest in the present discussion is that Agduhr describes a single muscle fibre which shows two hypolemmal end-plates upon it. One of these is degenerated and therefore the termination of a somatic nerve fibre. The other receives a non-medullated nerve fibre. In consequence of this Agduhr describes this as an instance of a single muscle fibre receiving both a somatic motor and a sympathetic termination. He records, however, that the plate regarded by him as being sympathetic in origin had a large extension on the muscle fibre, and on account of this approached in character the somatic motor terminal plates. Further, he states that the distance of the two plates from one another is "such as one finds in a spinal plurisegmental innervation of the separate muscle fibres." If we add to these considerations the short period of degeneration allowed in the specimen figured by Agduhr (five days), in the light of the fact that Boeke found that non-medullated fibres may be seen for a considerable period when actually undergoing degeneration, no proof is provided that the muscle fibre described is actually innervated by both somatic and sympathetic nerve endings. It is possible that the second plate is the ending of a non-medullated collateral of a medullated fibre rather than an independent sympathetic termination.

Kulchitsky, whose careful work stimulated the present investigation, has described, for instance, such additional plates provided by collaterals. Dr. Oliver Latham of Sydney has found a beautiful example of a non-medullated collateral of a medullated nerve fibre ending hypolemmally

in precisely the same way as the fibre which is figured by Agduhr. This collateral arises at a considerable distance from the site of termination of the medullated fibre, and the distance between the two plates is comparable to that which separates the endings in Agduhr's preparations.

Thus the contention of Boeke and of Agduhr that each muscle fibre receives a double innervation cannot be confirmed; nor does it agree with the evidence of other observers. Obviously the experimental method should afford evidence to settle the question whether individual muscle fibres are innervated by both somatic and sympathetic nerves. For this reason the muscles of the hind limbs of some of the goats employed by Dr. Royle for excision of the lumbar sympathetic chain on one side were histologically examined. The normal side was used for control. The preparations from the operated side showed in transverse section a number of small muscle fibres which were in marked contrast to the fibres of average size in the same preparation which were apparently normal. These diminutive fibres were too numerous to be accounted for as the conical or pointed terminations of normal muscle fibres. Further, the connective tissue (endomysium) between the individual muscle fibres has increased in amount. This was specially noticeable in a preparation secured by killing the animal six months after performing the operation.

Conversely, the somatic innervation may be removed and the sympathetic nerve supply to the striated muscle left intact. The wing of the domestic fowl readily provides a preparation of this kind. Both the anterior and posterior nerve roots of the lowest three cervical nerves were severed without injury to the cervical sympathetic ganglia or the white ramus communicans which emerges at the level of the first thoracic segment. In this way the wing was deprived of the major part of the somatic nerve supply. Seventy days later the bird was killed. *Post-mortem* examination revealed that the operation had been successfully performed and the cervical sympathetic trunk was found intact. On microscopic examination large numbers of degenerated medullated nerve fibres were found in a preparation from the flexor muscles of the forearm. These muscles when stained in haematoxylin and cut in transverse section showed a marked contrast to the corresponding muscles from precisely the same point of the normal wing, which were treated in exactly the same manner. The individual muscle fibres of the normal muscle showed remarkably little variation in size throughout the preparations. The muscle of the wing from which the somatic innervation had been removed was in marked contrast to this. Many of the fibres were normal in size or slightly larger than the largest fibres of the normal muscle. These appeared in groups, and some fasciculi were entirely composed of them. Other fibres showed a marked diminution in diameter to approximately 33 per cent. of that of the muscle fibres of average size. These also occurred in groups, and in some cases they formed entire fasciculi. Other fasciculi were made up of groups of both normal and diminutive fibres lying in juxtaposition. The connective tissue of the muscle was increased in amount; a few masses of young connective tissue cells were in evidence between the bundles, and the endomysium was slightly more developed than in the control specimen.

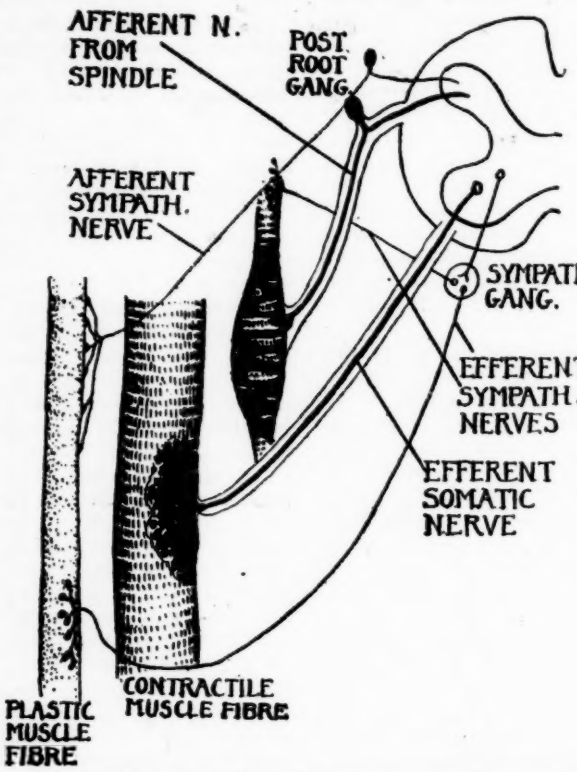

FIG. 2.—Diagram to indicate the three constituent elements of striated muscle, slender fibres, thick fibres, and muscle spindles, and their nervous connexions—efferent non-medullated fibre from sympathetic ganglion to slender fibre, afferent non-medullated fibre from slender fibre to the posterior root ganglion, medullated nerve from the spinal cord to the thick muscle fibre (whether or not it has an afferent fibre is not known), and muscle spindle, connected to posterior root ganglion by a medullated nerve and to the sympathetic ganglion by an efferent non-medullated nerve.

It is firmly established that diminution in the size of all the muscle fibres follows complete denervation. The work of Willard and Grau on white mice may be cited as evidence of this. The leg muscles on one side were deprived of their innervation. The corresponding muscles of the other side were used as a control. Specimens were investigated showing atrophy of from three to sixty-three days' duration. These authors found a progressive decrease in fibre diameter, this change being unaccompanied by any change in the number of fibres. They describe the decrease in diameter as being "up to 68 per cent." During the period under observation there was no connective tissue proliferation between the muscle fibres. The difference in the findings of Willard and Grau and our observations in regard to the proliferation of connective tissue is no doubt accounted for by the fact that a partly denervated muscle is not entirely inactive. In the case of sympathetic denervation, which removes plastic tone, increase of fibrous tissue no doubt renders the muscle more efficient in maintaining a position imposed by active movement or by selective reflex activity. When contractile tone is removed by somatic denervation only a very slight increase of connective tissue occurs over a period of sixty days. The support of the wing during this period had therefore been provided practically entirely by the healthy muscle fibres which exhibited plastic tone.

The conclusion can be drawn from the experimental facts that partial denervation—somatic or sympathetic—affects some fibres and not others. This confirms the evidence obtained from a study of gold chloride preparations. On the basis of innervation, therefore, skeletal muscle is found to consist of two sets of fibres. In the experimental preparations the fibres of each set tend to occur in groups. This is in accordance with the findings of Latham in gold chloride preparations of the skeletal muscle of the domestic fowl and goat, that a considerable number of fibres, either receiving the endings of only non-medullated fibres or of only medullated fibres, tend to occur in proximity to one another. Even in teased preparations a group of as many as seven muscle fibres may receive non-medullated fibres, while no terminations of medullated endings may be visible in the same field. Such an appearance may have been a source of error in the past in suggesting that every muscle fibre receives a sympathetic innervation. The possibility arises, therefore, that the mode of innervation of skeletal muscle is that any individual muscle fibre receives one motor nerve ending—somatic or sympathetic as the case may be—and not both motor endings, and that each type of muscle fibre occurs in groups.

As an instance of the presence of a single type of motor innervation it may be recalled that the muscle fibres of muscle spindles receive sympathetic motor terminations. The existence of a double motor innervation to these muscle fibres, although suggested by Perroncito, has not been confirmed. Recently, on the other hand, Agduhr has found sympathetic terminations on these intrafusal fibres in the limb muscles of the cat. Kulchitsky and Dart have found these endings in relation to the fibres of the muscle spindles of the python. Dart is in error in regarding the fibres as being finely medullated, but he shows clearly that the endings are associated with diminutive sole plates. This

type of ending has already been described in this lecture as being sympathetic efferent in character. The intrafusal fibres seem, therefore, to have a single efferent innervation which is sympathetic in origin. This is in itself a point of great importance, for it suggests that other fibres may be similarly innervated.

I suggest, therefore, that the skeletal muscle of vertebrates consists of two sets of muscle fibres disposed in groups each with its own specific innervation, and, consequently, its own specific function, the experimental investigation of which will be discussed in the next lecture. I shall then endeavour to establish the proof of the following claims. The fibres receiving somatic nerve endings are concerned in shortening as the result of voluntary and of reflex activity, and in isometric contraction during the continuation of the stimulation. These fibres are responsible, therefore, for voluntary movement and for what Langelaan[10] calls "contractile tone." They are comparable to what Sherrington calls the "movement muscles" of invertebrates. Other fibres, innervated by the sympathetic system, are first inhibited and then lengthened and shortened during these processes, and remain at the new length passively imposed upon them. In other words, these fibres exhibit what Sherrington has called "plastic tone." They are comparable to the "fixing muscles" of invertebrates. During movement these fibres aid in supporting the weight of the moving part. At the termination of movement they take part in maintaining the position attained as the result of the movement.

Further, some of the "fixing muscle" fibres appear to be included within a sheath to form the special sense organs (muscle spindles) which transmit afferent impulses to the central nervous system by means of somatic sensory fibres. The degree of lengthening and shortening imposed upon these intrafusal fibres is maintained by the influence of their sympathetic innervation. Epilemmal sympathetic endings found on other fibres that receive also a sympathetic motor innervation probably constitute in a similar way sympathetic proprioceptor organs.

All the evidence related above in regard to the exact mode and the function of the double innervation of skeletal muscle indicates that the differentiation of muscles into "movement" and "fixing muscles," which is so apparent in some invertebrates, is not lost in vertebrates. These two muscle systems of invertebrates are represented as small muscle groups which mingle together to constitute the skeletal muscles of vertebrates.

From the standpoint of physiology the existence of such an arrangement would readily account for the marked similarity in the behaviour of the "catch" mechanism of invertebrates and the activities of skeletal muscle in maintaining posture. In reviewing the points of comparison between the two processes, Bayliss recalls the suggestion of A. V. Hill that a more efficient mechanism of maintaining a weight must be exhibited under natural conditions than that displayed by the sartorius muscle of the frog when its nerve is stimulated by induction shocks. He compares also the process of inhibition of the "catch" mechanism (von Uexküll) with that of the tonic contraction of skeletal muscle during decerebrate rigidity (Sherrington); and he calls attention to the existence of an extremely low rate of metabolism during the activity of the "catch" mechanism (Parnas, Bethe) and of skeletal muscle in the state of decerebrate rigidity (Roaf, Lovatt Evans). If separate muscle fibres exist in vertebrate skeletal muscle for the fixation of posture and for active and reflex shortening and for isometric contractions of the muscle, the differences in metabolism and in the nature of inhibition during the manifestations of these activities of the two different muscle groups will be readily comprehended.

From the pathological standpoint this view introduces a line of attack to relate the symptomatology and pathology of such diseases as myotonia atrophica, the muscular dystrophies, and myasthenia gravis. In poliomyelitis it has long been known that sometimes the sympathetic ganglia partake of the degeneration found in the lower somatic motor neurone system. Such cases are, no doubt, those in which the tone of the muscle affected is completely lost. In other cases, in which there develop rapidly structural deformities due to the tendency for the limb to remain fixed in positions imposed upon it, the sympathetic innervation probably remains intact. Further, according to this conception, the operation of sympathetic ramisection for spastic paralysis consists in removing the nerve supply to the fixing muscle fibres only. All the movement fibres remain entirely unaffected. This, no doubt, accounts for the efficiency of the partly denervated muscles after long periods of time—for example, twelve months. On the other hand, according to the prevailing theory of the existence of the double innervation of each individual muscle fibre, the operation would entail partial loss of the nerve supply of each fibre of the muscle; or, more precisely, that the sarcoplasm of each muscle fibre is deprived of its nerve supply. This is unlikely in view of the unimpaired activities of reflex and of active shortening of the muscles which remain for long periods after the operation.

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PER-URETHRAL OPERATIONS FOR PROSTATIC OBSTRUCTION.

BY

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THE introduction of suprapubic prostatectomy in 1901 led almost immediately to the abandonment of the various palliative methods of treatment that had previously been tried with indifferent success. Since that time prostatectomy has grown in popularity, so that nowadays it is regarded by many surgeons as a routine method of treatment to be carried out in all cases of enlargement, except where some special contraindication exists. With improved technique the mortality of the operation has steadily diminished, so that patients who thirty years ago would certainly have been condemned to catheter life are now subjected to operation. But great as has been its success, prostatectomy must still be regarded as a serious operation and one that, even in the hands of the most competent, is attended by a comparatively high mortality rate. In view of this and of the fact that there must exist cases in which prostatectomy is unjustifiable, it may not be out of place to describe certain less drastic procedures which, in suitable cases, give excellent results.

Objections to Partial Removal of the Prostate.

Three criticisms are usually levelled against any operation that fails to effect a complete removal of an enlarged prostate: first, that the treatment is likely to fail in its object; second, that even when it succeeds the results are temporary; and third, that as an apparently innocent prostate may microscopically show malignant changes it is unwise not to remove it. The first two of these objections will be discussed more fully later. Suffice it for the moment to state that the whole success of partial operations on the prostate depends on a judicious selection of cases, and that failures are generally due to an ill advised attempt to tinker with a large prostate that is amenable only to total enucleation.

With regard to the third objection, as long ago as 1900 Albarran and Hallé called attention to the fact that a certain number of prostates which clinically appeared to be examples of innocent enlargement showed malignant changes when examined microscopically. This observation has since been confirmed and a figure approximating to 14 given as the percentage of cases in which these changes are found. From this it has been argued that as it is impossible to be sure that an enlarged prostate is pathologically innocent the wisest procedure in all cases is to carry out total prostatectomy. This argument for the wholesale removal of enlarged prostates would be unanswerable but for two facts—the first, that total prostatectomy itself has

a very high mortality rate; the second, that even if a prostate showing malignant changes be enucleated the patient runs a very considerable risk of a recurrence at some future date.

In balancing risks, therefore, it is necessary to remember that the diminished likelihood of future malignant trouble may in certain cases be more than neutralized by the immediate risk of prostatectomy. Exactly what that risk is depends on a great many factors, but at a large general London hospital I find that the mortality rate of prostatectomy for simple enlargement during the years 1914 to 1923 works out at 22 per cent. This is undoubtedly high when compared with the figures given by genito-urinary experts, but even the more favourable figures show that the death rate is above 5 per cent. It is therefore impossible to lose sight of the fact that prostatectomy must even nowadays be considered a dangerous operation. It is still more difficult to estimate the amount of protection from future malignant trouble afforded by total removal of the gland, but in this connexion it is interesting to note that of the five clinically innocent but pathologically malignant prostates recently removed by R. H. J. Swan two recurred in spite of the complete prostatectomy. It would therefore seem probable that too much emphasis has been laid on the argument that it is wise to remove all enlarged prostates simply on account of the risk of their being malignant, and, that being so, it is illogical to condemn offhand all partial prostatectomies.

Since failure to obtain satisfactory results from partial operations is generally due to faulty selection of cases, it is important to consider what forms of prostatic obstruction are suitable for such treatment. First amongst these suitable cases is that which is generally known as the prostatic bar.

Prostatic Bars.

It has long been recognized that there are patients who have all the symptoms commonly associated with prostatic obstruction and yet in whom rectal and even cystoscopic examination fails to reveal any enlargement. This condition has been termed variously, sclerosis of the internal sphincter, contracture of the vesical neck, atrophy of the prostate, atony of the senile bladder, or, in French, "prostatisme sans prostate," but the mechanism underlying the obstruction is the same. The symptoms resemble closely those due to prostatic enlargement—namely, frequency and urgency in micturition, difficulty or delay in starting, progressive diminution in the force and calibre of the stream, and an increasing amount of residual urine, but not infrequently these begin earlier in life than is usual with general enlargement of the prostate.

The diagnosis rests chiefly on an examination of the posterior urethra and vesical neck, the cause of the trouble being found in a bar formation which affects either the posterior vesical lip or the floor of the prostatic urethra, and by its presence obstructs the outflow of urine. Our knowledge of this form of prostatic obstruction has been considerably added to by the work of Alexander Randall, who, in a series of 300 necropsies, found 54 examples of prostatic bar. A careful examination of these 54 cases showed that they could be divided into four distinct types, two of them fibrous in nature and two glandular. A common type is that shown in Fig. 1. In this form of bar the posterior lip of the vesical orifice is thickened, raised, and stretched tightly across the outlet. Seen in sagittal section there is a diminution in the distance between the vesical orifice and the verumontanum, and an increase in that between the vesical lip and the perivesical tissue. It is undoubtedly due to an increase in fibrous tissue following some old-standing inflammatory process such as a chronic prostatitis. The second type described by Randall is similar to the first, except for the fact that the bar projects upwards so as to encroach on the trigone rather than forwards towards the prostatic urethra. In the third and fourth types the bar is glandular, being due to hypertrophy of the submucous glands of the posterior commissure in the one case, and of the glands of Albarran in the other. In none of these cases is the enlargement other than local, and yet the presence of hypertrophy of the bladder, trabeculation, etc., as associated conditions shows that a definite degree of obstruction exists.

FIG. 1.—Sagittal section through a prostate showing a common type of fibrous bar, with thickening of the posterior lip, increase in the distance between the vesical orifice and the perivesical tissues, and a diminution in that between the orifice and the verumontanum. The dotted line marks off the area removed by operation.

However, it may be urged that even if prostatic bars of this nature exist they are so rare that, from the point of view of the practical surgeon, their importance is negligible. I cannot agree with this criticism. It has been my experience that the more careful the preliminary investigations of patients presenting themselves for prostatic obstruction, the more important does this type of case appear. How frequently has one seen a bladder opened in the expectation that a prostate suitable for enucleation would be found, and in place of this a small fibrous gland discovered which could only be removed piecemeal by scissors and forceps! Most surgeons have memories of struggles with the "small fibrous type of prostate," and not a few of their patients have been returned to bed unrelieved of their obstruction. It is difficult to find figures that will give an accurate idea of the commonness of such cases, but probably the most comprehensive are those obtained from Randall's *post-mortem* room investigation. Out of 158 subjects showing prostatic abnormalities after death, 33.5 per cent. were examples of bar formation. As Randall's findings are very instructive in showing the relative frequency of other forms of prostatic enlargement his figures are appended:

Relative Frequency of Various Forms of Prostatic Enlargement (Randall).

800 Necropsies.	158 Prostatic Abnormalities.		
	Number.	Per cent.	
Lateral lobe hypertrophies alone	29	18.4	
Median lobe hypertrophies alone	42	26.7	
General trilobar hypertrophies	22	14.0	
Anterior lobe	3	1.9	
Carcinoma	8	5.0	
Sarcoma	1	0.6	
Median bar—fibrous	8	5.0	
" " glandular	13	8.2	33.5
" " not classified	18	11.4	
" " small and non-obstructive ...	14	8.9	
	53		
	158		

But the proof that bar formations are of frequent occurrence does not rest on *post-mortem* findings alone. J. R. Caulk, who has made a special study of this condition, reported in 1921 that of the cases consulting him for obstructive symptoms, 20 per cent. were examples of bars, and since that time, with a better understanding of the condition and improved methods of examination, he has found a considerably higher rate of incidence. My own observations, so far as they go, are in harmony with Caulk's.

Young's Punch Operation.

In 1909 H. H. Young devised an endoscopic tube by means of which prostatic bars could be removed through the urethra under local anaesthesia. Although the early instrument was in the form of a urethroscope, it was soon found that the operation of punching out an obstructing bar could be carried out efficiently without visual observation, and the original apparatus was abandoned in favour of one of simpler design (Fig. 2), consisting of an outer sheath with a window and an inner tube with a sharp cutting edge. It is employed as follows.

The bladder is distended with fluid and the instrument introduced with the inner tube pushed home so as to close the opening in the sheath. The instrument is then withdrawn with the opening uncovered so that on reaching the urethra it engages the prostatic bar, an event that is signalled by the sudden stoppage of the fluid escaping from the bladder. The operator then pushes home the inner cutting tube and excises a small fragment of

tissue. As a rule more than one cut is necessary, the foregoing proceeding being repeated, and two or three fragments removed in turn.

Since the introduction of this instrument various modifications have been made by different workers. The chief criticism levelled at Young's punch operation has been that it is liable to be followed by haemorrhage. However, although this is usually fairly brisk for a short time after operation, it does not often occasion any real anxiety. Personally I have not encountered difficulties in this direction, and my reason for abandoning Young's operation in favour of other procedures is that I have been dissatisfied with the small amount of tissue that can be conveniently removed by this method.

The Cautery Punch.

Four years ago J. R. Caulk introduced a punch in which the inner cutting tube acted at the same time as an electro-cautery, thereby reducing the risk of haemorrhage and increasing the amount of tissue removed by the operation.

In his instrument the cutting is done by a circular platino-iridium knife, carefully insulated from the rest of the instrument and heated to a dull red heat by a suitable electric current. It has been found unnecessary to provide water cooling for the outer tube since the whole of the burning operation can be carried out within the space of five or six seconds, and very little general heating occurs. In dividing the bar the inner tube is given a rotatory motion, and should it be considered advisable to increase the action of the cautery the tissue may be researed by pulling backwards the blade at half heat.

It is of the greatest importance in using Caulk's instrument to employ the right form of current, and reliable results can only be obtained by using an alternating current of about 10 volts and 150 ampères. This unfortunately limits the utility of the instrument, for the current available in different localities varies considerably, and will have to be modified in certain cases by the use of cumbersome and costly transformers. At any rate, after having made various efforts to overcome these electrical difficulties in London, I came to the conclusion that they constituted a serious objection to what was otherwise an excellent weapon, and I discarded Caulk's cautery punch in favour of the instrument described in the next paragraph.

The Diathermy Punch.

The instrument described below has been constructed for me by Mr. Schranz of the Genito-Urinary Manufacturing Company, to whose ingenuity in design I give full credit.

In external form it resembles the Young punch, but the sheath is constructed of bakerlite—an excellent insulating material allied to vulcanite—with a metallic lining. This lining only comes to the surface at the edge of the window situated below the beak; elsewhere it is insulated from the urethra by the bakerlite covering. (See Fig. 2.)

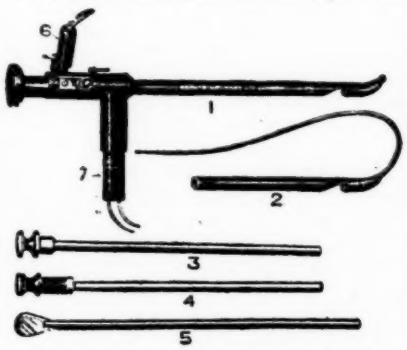

FIG. 2.—(1) Sheath with attachments—(6) for current to lamp, (7) diathermy apparatus; (2) bougie guide screwed to sheath; (3) obturator; (4) punch.

The punch is introduced with an obturator in position, and to facilitate its insertion in difficult cases a pliable bougie guide may be attached to the beak by means of a screw. When in position the obturator is withdrawn, and a system consisting of a telescope, a lamp, and inlet and outlet tubes for irrigation is substituted, thus converting the instrument into a posterior urethroscope of the Gueringé type. Under guidance of the eye the punch is now withdrawn until the bar is seen to engage the window. Once it is in position the instrument is held firmly and pressure exerted on the bar by slightly raising the proximal end. Connexion is then made with the source of diathermy current. Immediately the current is turned on the inner metallic layer of the sheath becomes a urethral electrode, but owing to the insulation conferred by the bakerlite covering the only point of contact with the urethra is the edge of the window engaging the bar. From this edge can be observed to spread a line of coagulation which within ten or fifteen seconds whitens the whole area of prostate engaged. By maintaining continuous irrigation the process of diathermy can be carried out under complete ocular control, the current being turned off when it is judged that a sufficient depth of tissue has been destroyed.

In order to remove the wedge of destroyed tissue and thus obtain immediate benefit without waiting for the separation of sloughs, the telescope and lighting system is withdrawn and an inner tube, resembling that used in the Young punch, inserted. By pushing this home and giving it at the same time a slight rotatory movement, a core of destroyed tissue about 2 cm. in length and 0.5 cm. in diameter may be removed. This does not, however, constitute the whole amount of prostatic tissue destroyed by the operation, the full benefit of which will only be obtained at a later period when the sloughs have separated. The telescope and lighting system may then be reintroduced, the area of operation inspected, and a decision arrived at as to whether any further fulguration is required. If the operator is satisfied with the amount of tissue dealt with, the bladder is emptied, the obturator inserted, and the instrument withdrawn.

It is obvious that the diathermy punch briefly described above offers many advantages over both Hampton Young's and J. R. Caulk's instruments, the chief of these being the avoidance of all possibility of haemorrhage by the use of diathermy, and the fact that the whole operation is conducted from beginning to end under the guidance of the eye. The use of the inner punching tube gives the same immediate advantages conferred by Young's instrument, and besides this there is the diathermy action, which adds so much to the efficacy of the operation, and which is absent in Young's procedure. It is, indeed, not unusual for a patient with complete retention to empty his bladder a few hours after treatment with the diathermy punch and for his stream to improve still further with the subsequent separation of sloughs. Minor improvements are still being made in the instrument.

Fulguration of the Prostate.

As an alternative to the punch operation prostatic bars may be dealt with through a posterior urethroscope carrying a diathermy electrode. This form of treatment has been championed in France by Luys, and in suitable cases gives good results. In England the method has on the whole been neglected, and for this reason no apology will be offered for dealing with it at some length.

Any good operating posterior urethroscope may be used for the purpose of fulguration. My own practice is to employ two instruments—a direct-vision endoscope of the Gueringé or Joly type and the new prismatic operating cysto-urethroscope made by Wolfe. With the former I map out my field of operation, since it gives a clearer idea of the topography of the posterior urethra and of the mechanics of the obstruction than does the prismatic instrument. Having achieved this, I discard the direct-view urethroscope in favour of the Wolfe cysto-urethroscope, which has the great advantage of permitting the use of a larger electrode (No. 8) than the Gueringé instrument, and of thus shortening the time consumed by the operation. A current of from 500 to 2,000 milliampères is used, and throughout the whole operation a copious stream of fluid is kept circulating through the instrument in order that a clear field of vision may be maintained. The position of the patient, the general arrangements of the theatre, and the preliminary stages of the operation are the same as for a cystoscopy. A general anaesthetic is as a rule required, but in certain cases where this has been inadvisable I have operated under local anaesthesia, the prostate being infiltrated with novocain through a needle mounted on the end of a ureteric catheter. In any case, once the instrument has been introduced and fulguration started only light anaesthesia will be required. Special arrangements must be made to allow of continuous irrigation throughout the operation, and for this purpose a large-sized irrigator should be used and kept filled. In those cases in which a preliminary suprapubic drainage has been carried out there is, of course, no danger of overdistension of the bladder, and all that is necessary is to attach a length of rubber tubing by means of a glass connecting piece to the suprapubic drain and to lead the irrigating fluid into a bath placed beneath the operating table. In all other cases, however, the operator will have to keep an eye on the condition of the bladder throughout the operation, and at intervals evacuate.

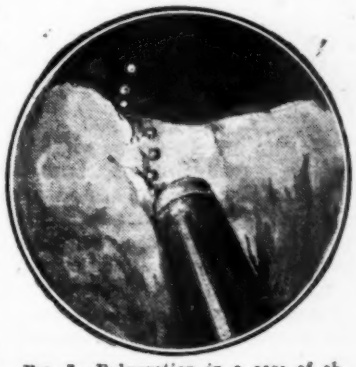

FIG. 3.—Fulguration in a case of obstruction due to bar formation. In addition to the bar there exists a minor degree of enlargement of the right lobe.

It is usually more convenient to start fulguration at the bladder neck and to work towards the urethra. The method

is the same as in dealing with bladder growths, and the chief difficulties likely to be encountered are those associated with keeping a clear field and with recognizing the exact position of the operating electrode. Obscuring of the visual field may be due to blood, debris from the destroyed tissue, and bubbles of hydrogen given off at the electrode. The first two obstacles to clear vision are removed by irrigation, and the third by rotating the instrument so that the bubbles may escape to the upper part of the field. The time required to complete the operation depends on the amount of prostate requiring removal, the strength of current, and the calibre of the electrode; it may be as much as an hour where a large amount of tissue has to be dealt with. In the simplest type of prostatic bar all that is necessary is to cut a channel through the ridge so that the level of the bladder neck is flush with that of the urethra, and to widen and deepen the channel sufficiently to provide a free exit for the urine. When some lateral enlargement is associated with the bar formation it will be necessary to deal with this as well (see Fig. 3). As in the case of the bar, the portion of the lateral lobes nearest the bladder should be attacked first, the optical end of the instrument being raised so that the electrode lies in the lower part of the cleft between the enlarged lateral lobes. Energetic treatment of the adenomatous masses will be necessary if the result is to be satisfactory. Both sides should be attacked at the same sitting, each portion of the obstructing lobes being brought in turn under treatment from the level of the bladder neck outwards towards the verumontanum. Where more than a slight degree of enlargement exists several sittings may be required.

Very little after-treatment is necessary, and it is not my practice to tie in a catheter. A most remarkable point is the complete absence of pain or haemorrhage, or indeed of any other complication, after this operation. What little bleeding there is at this early period is usually due to the trauma of instrumentation, and ceases within a few hours. Most patients are able to get up and, if necessary, to leave hospital or nursing home the day following the operation, and their discomfort is certainly not greater than that commonly experienced after a cystoscopy. An alteration in the stream is not usually noted for four or six days, but as sloughs begin to come away an improvement occurs. The separation of these sloughs may be encouraged by posterior irrigation according to Janet's method. In those cases in which more than one sitting is required an interval of three to four weeks should be allowed between operations so as to allow time for the separation of the sloughs and the subsidence of the reaction. The procedure at the second or third operation is exactly similar to that of the first, and as a rule it will be found that the work is considerably easier than on the previous occasions. In the majority of my own cases two sittings have been sufficient.

SUMMARY AND CONCLUSIONS.

It has already been stated that the methods of treatment described in this paper are only to be regarded as a substitute for prostatectomy in certain types of case, and that diathermy or punch operations, used indiscriminately, are bound to lead to disappointment and failure. The type of case for which these methods are eminently suitable is the prostatic bar, but fulguration may also be used against the lesser degrees of glandular enlargement when for one reason or another prostatectomy is difficult or dangerous. If, however, the prostate be considerably enlarged the results obtained will in all probability be incomplete and transitory. Fulguration may also be employed, but with less certainty of success, in cases of post-operative obstruction, and in cases of malignant disease of the hard sclerotic kind.

For the sake of convenience the conclusions reached in the foregoing paper will be tabulated as follows:

1. Total prostatectomy, in spite of a fall in mortality, must still be regarded as a serious operation and one that in many cases is attended with very considerable risks.

2. The arguments for removing all enlarged prostates as a precautionary measure against malignant disease are not entirely sound, since enucleation of a gland showing malignant changes is no guarantee against recurrence, and the immediate risk of a prostatectomy may in some cases be greater than the remote risk of malignancy.

3. Treatment by punch operations and by fulguration is of the greatest use in certain types of obstruction. These procedures are particularly suited to cases of prostatic bar, and of minor enlargement where enucleation, owing to the existence of complications, would be difficult or dangerous.

4. Success in the use of these minor operations depends on the exercise of a nice discrimination in the selection of cases. For this reason the very greatest care and thoroughness must be employed in the preliminary investigation. Otherwise the results will be uncertain and short-lived.

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THE SURGICAL TREATMENT OF CHRONIC ULCERATIVE COLITIS.

BY

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THE medical treatment of chronic ulcerative colitis has, in my experience, proved extremely unsatisfactory. The ordinary intestinal antiseptics by the mouth are practically useless, and the great difficulty is to determine when active surgical interference should be decided upon. There is no hard and fast line in determining when cases of colitis reach the chronic stage.

There is no doubt that a large percentage of Indian troops are "carriers" of dysentery, especially of the amoebic form; in peace, when they are comfortably housed in cantonments where they have their normal rations and are not exposed to hardships and fatigues, they suffer little or no inconvenience. The same men under field service conditions, when their rations vary in quality and quantity, usually for the worse, and especially when they are exposed to cold and wet weather, soon manifest symptoms of colitis.

My own experience of these cases has been gained under field service conditions on the north-east frontier of India, 1911-13, and in Mesopotamia during the recent war. During the operations on the north-east frontier *post-mortem* examinations in cases of colitis revealed the intense and extensive ulceration of the large intestine which can occur; the conditions impressed me with the utter hopelessness of medical treatment. It must be remembered that Indian troops are far more susceptible to scurvy than European troops; in my experience of active service, it takes about three months from the beginning of a campaign for the disease to show itself. The presence of scurvy or of a tendency to it is a very serious complication in cases of colitis and seriously aggravates the condition.

During the recent war (1914-18), while acting as surgeon to an improvised hospital for Turkish prisoners in Mesopotamia, and later as civil surgeon of Baghdad, an opportunity arose of observing a large number of cases of colitis; the majority were chronic and were complicated by scurvy. The combination of these two diseases made the colitis extremely intractable, and in consequence large numbers died.

While in Mesopotamia I performed appendicostomy for intractable ulcerative colitis in ten patients—Turks, Arabs, and Indians. They were all extreme cases, and all were complicated with scurvy in its various stages. The operations were performed under chloroform, and owing to the emaciated condition of the subjects the appendix was easily and rapidly approached in all cases. In the first two cases the appendix was not opened for two days, but in the remaining eight it was opened at the time of the operation, and lavage carried out at the same time, the wound being sealed with collodion. After a few irrigations of the large intestine the patients began to show remarkable improvement. The cachectic appearance due to the intestinal auto-intoxication disappeared and the general condition improved

beyond all expectations. All these cases were operated upon under very adverse conditions; there were no nurses or trained attendants to help, and there was great difficulty in obtaining fresh food and milk.

The immediate results were excellent, but it became obvious that lavage had to be persisted in for prolonged periods to maintain these results, and great care had to be taken with regard to diet. Any indiscretion in diet brought on a recurrence of the symptoms. In practically all cases eusol was used until the washings were clean, and then ordinary saline solution. The main factor was the quantity used, several pints at each lavage being essential. I found that the introduction of a rectal tube for the evacuations was a great relief to the patient.

I regret that I am unable to give the after-history of these patients as they were transferred to other hospitals, and in those days, owing to the scarcity of medical staff, no accurate records could be kept. The immediate results in all cases were excellent, and this method of treatment impressed me as being the only rational procedure.

Later on I had an opportunity, while surgeon to the Bowring Hospital, Bangalore, of observing and treating a case in more advantageous circumstances.

Nallandi, aged 34, was admitted to hospital on June 26th, 1920, suffering from chronic dysentery. He had had an attack of dysentery in February, 1920, from which he recovered; in April, 1920, he was operated upon for haemorrhoids at another hospital.

Condition on Admission.—He was extremely emaciated and anaemic, with sunken abdominal walls. There was extreme tenderness over the left iliac region. The liver and spleen were normal in size. The motions numbered about fifty daily; they consisted of blood-stained mucus and little faecal material. Microscopical examination showed *Entamœba histolytica* and cysts to be present.

On June 28th an ipecacuanha bolus was divided into two parts—one was given at 5 a.m. and the other at 2 p.m., preceded by an opium draught. Stools during the twenty-four hours numbered thirty-five. The next day emetine, grain 1/2 bis in die, was given, and the stools became less frequent and less bloody.

On July 1st the patient was examined, under chloroform, with the sigmoidoscope. Several small ulcers were seen high up, and two large ulcers, oval in shape with well defined edges, the bases covered with muco-pus; the intervening mucous surface was reddish and granular in appearance and was covered with muco-pus. The emetine injections were continued up to July 6th. By this time the stools were less frequent, but the general condition of the patient was much worse.

On July 7th he was operated upon with a view of doing an appendicostomy. The appendix was found to be atrophied and useless for this purpose, so a valvular caecostomy was done, a No. 12 rubber catheter being introduced and the bowel washed out with saline solution at the conclusion of the operation.

On July 12th the patient was doing well: the stools numbered two daily; there was no blood or mucus; temperature normal. On July 19th the general condition began to improve, the septic appearance vanishing and diet being gradually increased with solids—ovaltine and biscuits. Injections of emetine, grain 1, were commenced on July 30th and given for five days as a precaution, the bowel being still washed out twice daily.

The bowel was washed out twice daily until November 13th, and by this time the patient was practically on a full diet. Three days after the irrigation ceased the fistula was almost closed, and the patient was discharged cured on December 28th, 1920, having increased over 2 st. in weight. A final sigmoidoscopic examination showed all the ulcers healed.

Conclusions.

Cases of colitis persisting over two or three months without intermission and with profound emaciation and anaemia which do not respond to medical treatment should be handed over to the surgeon. When they reach this stage they are invariably examples of mixed infection, whatever the organism that caused the primary infection may have been. In severe cases the large intestine may be likened to an abscess or bag of pus.

In all chronic cases a sigmoidoscopic examination should be made. In the case recorded above sigmoidoscopic examination showed the existence of extensive ulceration of the lower bowel and confirmed the diagnosis of chronic ulcerative colitis.

The only rational treatment, when the diagnosis is confirmed, is frequent irrigation of the large intestine, either through an appendicostomy or a valvular caecostomy, with eusol solution until the washings are clean; after that, with large quantities of warm saline for prolonged periods.

Valvular caecostomy is particularly mentioned, as the closing of an ordinary caecostomy may be difficult later, besides being unpleasant for the patient and for those associated with him.

ENTAMOEBAE AS AN ETIOLOGICAL FACTOR IN MULTIPLE ABSCESSES.

BY

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AND

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MULTIPLE deep abscesses are of frequent occurrence in Nigeria. There are two main clinical types: (*a*) those which affect the muscles of the limbs, and (*b*) those which affect the ilio-psoas muscle in particular; this type is usually single.

Although infection with *Filaria bancrofti*, *Dracunculus medinensis*, and pyogenic organisms may explain the minority, the etiology of the majority remains a problem.[1]

Entamobae are frequently the causative factor in hepatic abscess; they have also been reported as occurring in abscesses of the jaw, the brain,[2] the spleen, and salivary glands.[3] Still more recently Captain S. Smith[4] has recorded their appearance in the left knee-joint; the patient in whom this occurred was given emetine and made an uninterrupted recovery.

We would suggest that in view of the various organs of the body in which entamoebae have been found, there is reason to believe that amoebic pyaemia is more than a possibility.

Jonah, an African male aged 26, cook in a European household, was admitted into hospital on May 26th, 1924, with a history of fever and pains which had lasted for three days. He looked extremely ill; the eyes were sunken and the tongue furred. On admission the temperature was 104° and the pulse 100. He gave no history of diarrhoea or dysentery, and stated that there had been no dysentery among the other boys in the household.

The temperature for the first two weeks after admission fluctuated between 100° and 104°. On May 31st a swelling in the left arm was opened and pus evacuated. Following this there were remissions of the temperature to normal, but it still rose to 104° in the evenings. On June 11th three more abscesses were opened—one in the right upper arm, one in the right thigh, one in the left buttock. The temperature remained high until June 30th, when a large abscess in the muscles of the back was opened. Entamoebae were present in this pus, and 8 grains of emetine were given in daily doses of 1 grain—commencing on July 2nd. On July 1st the temperature was normal, and so remained until July 9th, when the final dose of emetine was given. On the evening of this day it rose to 101°, and again to this level on July 15th. All the abscesses healed rapidly, and there is now (July 21st) only a very little watery discharge from the wound in the back.

Pathological Report.

On June 30th the following facts were recorded:

Examination of Urine.—Nothing pathological detected.

Examination of Faeces.—The ova of *Ascaris lumbricoides* were present in small numbers. No ova of *Ancylostoma duodenale* were seen. No cysts or free living amoebae were noticed.

Examination of Blood.—Blood smear: No parasites found. Blood count: Red corpuscles 2,244,000 per cubic millimetre, white corpuscles 4,375 per cubic millimetre. Differential count: Polymorphonuclear 54 per cent., eosinophil 5 per cent., lymphocyte 34 per cent., large mononuclear 7 per cent.

Examination of Pus.—This was collected in the operating theatre under aseptic conditions during the incision of the abscess situated in the muscles of the back. The following points were noted: (*a*) The pus contained more red cells than one was led to expect from a macroscopic examination; (*b*) a large number of phagocytic leucocytes and endothelial cells of considerable size were seen; (*c*) the pus was sterile; but (*d*) entamoebae were seen. The pus from the same abscess was again examined on July 2nd and was found to show the same characters as on the previous occasion. Fresh pus was on this occasion examined in warm normal saline and also in warm Weigert's iodine, and the following facts were noted: Entamoebae were present; they showed active amoeboid movements, but less active than those exhibited by similar entamoebae in faeces; they were definitely phagocytic and were seen to ingest red corpuscles as well as pus cells. There was a distinct differentiation between the ectoplasm and the endoplasm. The endoplasm was vacuolated and contained in some instances six red cells. In the iodine preparation a distinct nucleus could be seen. Stained preparations showed the same histological characteristics. On July 3rd entamoebae were again found. On this occasion the pus was taken from another abscess located in the right upper arm. Entamoebae were again found in the pus on July 4th.

The serious illness of the patient, the condition of the blood, the physical peculiarities and the sterility of the pus,

the rapidity of the healing of the abscesses, point to a peculiar cause.

The presence of amoebae in sterile pus suggests: (1) That amoebae may play an important part in the production of deep abscesses found in this country. (2) That the pus from such abscesses should as a routine be examined for protozoa, with a view to establishing a causal relationship should such exist.

This case is published by the courtesy of Mr. W. R. Parkinson, F.R.C.S., surgical specialist, Lagos Hospital, Nigeria.

REFERENCES.
[1] Castellani and Chalmers (1919), p. 1836. [2] Manson's *Tropical Diseases* (seventh edition), p. 466. [3] Castellani and Chalmers (1919), p. 1828. [4] *Journal of the R.A.M.C.*, June, 1924.

THE NATURAL DURATION OF CANCER.

BY

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IN August, 1924, Lazarus-Barlow and Leeming[1] published statistics of the natural duration of cancer, based upon the experience of the Middlesex Hospital. They stated that similar data were being obtained from other hospitals, and the following are the results of an examination of the records of the Cancer Hospital for the years 1900-24. The figures in the two series agree very closely, and it would appear, therefore, that they are in all probability approximately accurate. What deviation there may be from the true value cannot be estimated on account of two errors which, of necessity, enter into the calculation. First, in all cases there is a latent period, of unknown duration and certainly varying from case to case within very wide limits, wherein the developing cancer produces neither symptoms nor signs. Secondly, the date of onset is that stated by the patient, whose memory is not always reliable and observation not always accurate. The former will entail an underestimate of the cancer's duration; the latter may introduce an error in either direction.

The cases dealt with in this paper include only a few sites—namely, rectum, breast, cervix, oesophagus, larynx, thyroid, and neck. The numbers of the first four are sufficient to make them of some value in themselves; those of the last three cannot do so, but may be of service if correlated with the figures from other institutions. In every case the duration is estimated from the reputed date of onset, and in most cases to the known date of death, but in a few instances where the patient left the hospital the length of life after discharge has been estimated by a careful consideration of the general condition of the patient and the extent of the disease when last seen, and I am convinced that no material error is introduced in this way.

None of the cases included received any but palliative treatment, which, as regards the rectum, includes colostomy. The impression gained by watching cases of rectal cancer in which colostomy has been performed is that the operation definitely prolongs life; if this were so the figures in Table II would contain an error accordingly. But when an attempt is made to estimate the increase of life no such increase is discovered (Table I).

TABLE I.—*Cases of Rectal Cancer.*

	Males.		Females.	
	No. of Cases.	Duration in Months.	No. of Cases.	Duration in Months.
Colostomy... ...	187	28	132	29
No colostomy ...	69	28	62	29

A number of the cases of cancer of the cervix received applications of radium, and are shown separately in Table II. Although the stomach is one of the organs most frequently affected by cancer, I have not included figures in relation to it because determination of the date of onset is so extremely difficult as to give rise, probably, to great error; because it is commonly treated by gastro-enterostomy, which again in a considerable proportion of cases materially prolongs the life of the patient; and because no great number of such patients actually die in the hospital. Carcinoma of any particular site other than those mentioned

[1] BRITISH MEDICAL JOURNAL, 1924, ii, p. 266.

TABLE II.—*Duration of Life in Cases which received Palliative Treatment only.*

Site and Age.	No. of Cases.	Natural Duration in Months.			Percentage of Cases below Mean.
		Mean.	Maximum.	Minimum.	
BREAST.					
Under 25	1	72	—	—	—
25-34	28	28	240	5	85
35-44	56	60	372	4	75
45-54	89	31	240	2	70
55-64	68	31	120	3	58
65-74	53	40	144	4	62
75 and over ...	16	30	78	5	70
Total	311	37	372	2	—
RECTUM.					
Under 25	10	36	216	6	80
25-34	29	46	372	6	69
35-44	56	27	156	4	67
45-54	125	28	180	3	62
55-64	136	29	211	2	62
65-74	81	24	90	3	52
75 and over ...	13	26	60	2	54
Total	450	29	372	2	—
OESOPHAGUS.					
Under 25	1	23	—	—	—
25-34	2	45	75	16	50
35-44	24	16	72	2	66
45-54	70	10	38	3	45
55-64	80	9	46	2	43
65-74	28	6	27	3	36
75 and over ...	5	14	40	2	40
Total	210	10	75	2	—
LARYNX.					
25-34	2	8	10	6	50
35-44	6	8	10	3	50
45-54	17	16	62	5	60
55-64	25	14	43	6	52
65-74	2	15	15	13	50
Total	52	14	62	3	—
THYROID.					
Under 25	1 (2)	6 (183)	—	—	—
25-34	—	—	—	—	—
35-44	1	9	—	—	—
45-54	5	16	48	4	60
55-64	2	4	5	3	50
65-74	3	9	27	5	66
75 and over ...	1	3	—	—	—
Total	13 (14)	10 (34)	48	3	—
CERVIX.					
Palliative—					
Under 25	3	15	20	9	66
25-34	29	14	90	4	62
35-44	112	15	78	2	64
45-54	171	16	168	2	59
55-64	86	17	96	2	66
65-74	36	18	108	3	61
75 and over ...	3	12	27	2	66
Total	440	17	168	2	—
Radium—					
Under 25	—	—	—	—	—
25-34	10	38	202	5	80
35-44	22	18	76	7	60
45-54	33	20	66	5	54
55-64	18	22	84	6	78
65-74	10	17	41	5	60
75 and over ...	1	28	—	—	—
Total	94	23	202	5	—
Totals (cervix)—					
Under 25	3	15	20	9	66
25-34	41	20	202	4	63
35-44	139	15	78	2	62
45-54	222	17	168	2	58
55-64	109	18	96	2	69
65-74	46	18	108	3	60
75 and over ...	4	16	27	2	50
Total	554	18	202	2	—
NECK.					
Under 25	1	9	—	—	—
25-34	1	12	—	—	—
35-44	1	48	—	—	—
45-54	7	8	12	4	43
55-64	5	17	36	5	80
65-74	3	20	31	12	30
Total	18	15	36	4	—

is relatively infrequent, and the numbers available for analysis so small as not to repay the labour of extraction.

The duration of mammary cancer is markedly longer than that of any other cancer, while that of rectal cancer comes easily second. It is interesting to note that in carcinoma of the cervix radium treatment seems to prolong life by an average of six months, thus confirming the general opinion amongst surgeons that it is of service in these cases. Only five cases of cancer of the breast were treated in this way, but of twenty-eight cases treated by *x* rays the average duration was only twenty-four months (against thirty-nine months for untreated cases), and each of the age periods showed a corresponding deficit.

The section of the table which deals with the thyroid gland contains, it will be noticed, bracketed figures; these include one case the duration of which was stated to be thirty years. The patient eventually died in the Cancer Hospital and the malignant nature of the growth was proved, but I find it difficult to believe that it was originally malignant, and consider it probable that malignant change ensued in a previously innocent tumour.

The excess of twenty in the total cases of cancer of the cervix over the combined totals treated by palliative measures and by radium is due to the fact that the former number contains twenty cases which received *x*-ray or combined *x*-ray and radium treatment.

Although the figures given by Lazarus-Barlow and Leeming lend support to the idea that cancer is more malignant in younger than in older persons, my figures afford no confirmation for such an opinion, and the duration of life appears fairly uniform throughout all the age periods.

For those sites which may be affected in either sex my figures give little, if any, indication that the duration of life is greater in females than in males.

TABLE III.—*Oesophagus, Larynx, Thyroid, Rectum.*

	Males.		Females.	
	No. of Cases.	Duration in Months.	No. of Cases.	Duration in Months.
Oesophagus ...	171	9	36	12
Larynx	44	14	8	14
Thyroid	4	17	9	8*
Rectum	256	28	194	29

* If the case mentioned in the text, and said to have had a duration of thirty years, be included, this figure becomes 43. There was no case in the series of malignant cervical glands in a female.

In conclusion, my thanks are due to Miss Hewer of the Cancer Hospital, by whom was performed the labour of searching the hospital records and abstracting the notes of over 2,000 cases, which form the basis of these statistics.

THE USE OF INTARVIN IN DIABETES MELLITUS.

BY

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THAT the fatty acids are broken down during oxidation by the removal of two terminal carbon atoms at a time (β-oxidation) seems to be fairly well established. Knoop[1] in 1904, in his classical investigations on the fate of the phenyl fatty acids in the animal organism, was the first to propound this theory, which was ridiculed by the pure chemists until Dakin[2] was able to show that an analogous breakdown of these and similar substances could take place *in vitro*. The investigations here referred to were all concerned with the oxidation of fats with an *even* number of carbon atoms in their molecule. Höckendorf[3] had earlier shown that the introduction into the diabetic organism of alcohols containing an *odd* number of carbon atoms, and which therefore on oxidation probably give rise to fatty acids with an uneven number of carbon atoms, brought about an increased excretion of glucose. Embden and his co-workers[4] found that on perfusing freshly excised liver with the lower members of the fatty acid series only those containing an *even* number of carbon atoms in their molecule yielded aceto-acetic acid as a result of their katabolism. Then Ringer and Jonas,[5] working with homologues of propionic acid, showed that valeric and hepatylic acids yielded glucose in the diabetic organism roughly in proportion to the amount of propionic acid that would be found as the result of β-oxidation, while butyric and caproic acids led to the increased excretion of aceto-acetic acid without increased glucose elimination. It was then that Ringer conceived the idea of looking for a suitable odd-carbon atom fat to be used in the treatment of acidosis, and which could be given in considerable amounts to diabetics without the possibility or danger of acidosis arising. His attempts, however, to synthetize such a suitable fat were unsuccessful. Other writers—namely, Janeway and Mosenthal—also saw the possibilities. Max Kahn succeeded in synthetizing a suitable fat—$C_{16}H_{33}COOH$, margaric acid—which is easily purified and which, when united with glycerol, forms a neutral fat. This compound, commercially known as intarvin, apparently can be katabolized in the organism without yielding ketogenic derivatives. Kahn states[6] that on administration to phlorizinized dogs this fat causes a marked increase in glucose elimination in the urine, showing that in the dog it is broken down to propionic acid and then converted into glucose in accordance with the theory brought forward by Ringer and his collaborators. In this communication he brings forward a certain amount of data to confirm his claims of the previous year.[7] He recommends the use of this fat in those cases where insulin has no apparent or only a moderate effect up to the extent of 1,000 calories daily from such a source. He also gives data from a patient with diabetes complicated by tuberculosis, who was fed with intarvin. In this subject it was found that 100 grams of intarvin given in addition to the ordinary diabetic diet caused no ketonuria, and the carbon dioxide combining power of the blood increased, while the blood sugar during this time remained below the threshold value. In December, 1923, he published[8] a more extensive series of observations to substantiate his claim.

In his first case, one of starvation acidosis, he found that the addition of 100 grams of intarvin caused the acetone to disappear, while on replacing it the next day with an equal weight of butter the acetonuria returned once more, to disappear next day on administration of the same quantity of intarvin.

In his second case, showing acetonuria and acetonaemia even on a low fat diet, the addition of 90 grams of intarvin cleared the acetonuria, and when an aliquot portion of intarvin was given the next day the acetonaemia also went. The patient's blood sugar also came nearer to a normal level.

In the third case it was found that when the patient had been rendered ketone-free without intarvin, 150 grams of this substance could be added to his diet without ketonuria reappearing, and the bicarbonate reserve of the blood increased while the blood sugar remained normal.

In the fourth case, one of pituitary glycosuria where insulin was ineffectual, feeding with the odd-carbon fat cleared up the acetone and brought about a gain in weight and the glycosuria was controlled. When the intarvin was omitted, ketonuria and glycosuria immediately reappeared.

Joslin[9] also reports favourably upon this fat. He states:

"It would appear to be ideal in the instance of acidosis in the presence of infection"; and "It can logically be recommended to those cases with whom the fat-carbohydrate ratio is above 3:1."

French workers[10] have also brought forward evidence in favour of the antiketogenic function of intarvin. There are, however, certain dissentients. Sevringhaus[11] reports that after comparing the effect of ingestion of beef suet and an equal quantity of glyceryl margarate in a ketonuria following carbohydrate starvation—

"Glyceryl margarate seems to have little or no ketolytic value aside from its glycerol content. It does not exert a protein sparing action in these diets."

Allen[12] condemns the use of artificial foodstuffs, including intarvin, in endeavouring to provide weight and strength for the diabetic, as for ever fruitless, being based on a superficial and false conception of the disease.

In view of this it seemed of interest to see what could be done with intarvin in cases of diabetes mellitus under treatment. Some of the relationships of intarvin to metabolism were consequently determined in four diabetic subjects, and, in addition, the effect of the absorption of intarvin on the blood fats was determined in two normal individuals.

Regarding the palatability of the fat, individual taste varies, but in our experience it was always found to be disagreeable, more especially if taken along with other food. Probably the mineral oil with which it is mixed when prepared for commerce had something to do with this. There is, however, no doubt that the intarvin is absorbed; both Kahn and Sevringhaus (loc. cit.) showed it to be 97 per cent. absorbed—that is, practically as well as beef suet would be under similar conditions. Considering that it was supposed to be antiketogenic in function it was thought worth while to determine whether the absorption of intarvin had any effect on the blood sugar.

Intarvin in Normal Subjects.

The following table shows the response of one of the normals (W. A.) to 250 c.cm. of 40 per cent. cream on a fasting stomach.

TABLE I.—*Response to Ingestion of 250 c.cm. of 40 per cent. Cream. (Normal Subject.)*

	Hours after Ingestion.					
	0	1	2	3½	5	6½
Total blood fat in mg. per cent. (Bloor)	455	435	411	504	460	414
Blood sugar in mg. per cent. ...	97	106	97	101	111	—
Respiratory quotient	0.90	0.80	0.78	0.79	0.81	0.80

Figures for cholesterol (whole blood) and for the metabolic rate showed no significant change and are not recorded. The maximum rise in the blood fat occurs earlier than in the cases reported by Cowie and Hoag,[13] otherwise there is little to note. The intarvin absorption curve of this subject shows similar features. On one occasion the intarvin, to render it more palatable, was made up with washed bran. The blood cholesterol showed a considerable rise at the end of the first hour (accountable for by the sterol content of the bran itself). Details being more complete as regards the intarvin absorption curve of the other normal (W. R.), the results are given in Table II.

TABLE II.—*Response to 100 grams of Intarvin.* (*Normal Subject.*)

	Hours after Ingestion.					
	0	1	2	3½	5	6½
Total blood fat in mg. per cent. (Bloor)	770	806	881	916	823	818
Blood sugar in mg. per cent. ...	103	125	118	114	108	125
Respiratory quotient	0.76	0.74	0.79	0.79	0.81	0.82

Blood cholesterol and metabolism showed no significant changes. In both cases it was found that the ingestion of the intarvin resulted at the end of the experiment in a certain looseness of the bowels, due probably to the mineral oil it contains. At no time did the respiratory quotient give any indication of the burning of carbohydrate in excess of normal. The urine in both cases did not show any abnormal constituents.

Intarvin in Diabetic Subjects.

In order to determine accurately the value of intarvin in diabetes it was considered desirable to have the diets of the patients stabilized at the highest possible level within the limit of tolerance, and then to add a quantity of intarvin. Patients were treated on the lines described in the BRITISH MEDICAL JOURNAL, August 23rd, 1924.[14] The diabetic cases were as follows:

CASE I.

A. R., aged 31. History of diabetes for four months. Admitted February 20th, 1924. On a diet of 1,240 calories ("B 12": carbohydrate 64.5 grams, protein 47.5 grams, fat 89 grams. G : FA : : 1 : 1) the patient excreted 41 grams of glucose in twenty-four hours. Acetone positive in the urine. The acetone and sugar were both got rid of from the urine by dieting alone, and the diet was built up to 2,400 calories. On March 12th on this diet it was found that the blood acetone (Van Slyke and Fitz) rose to 100 mg. per cent. two hours after breakfast, falling to 9 mg. per cent. two hours later, and remaining at this level for another two hours. Next day, similar conditions showed a fasting blood acetone of 5.8 mg. per cent., rising to 640 mg. two hours after a breakfast to which 100 grams of intarvin had been added, and falling to 36 mg. per cent. two hours later, to rise at the fourth hour to 42 mg. The blood fats showed a certain parallelism, but rose again at the end of the period. There was definite acetonuria, 364 mg. per cent. of acetone being present in the urine passed four hours after the intarvin.

CASE II.

W. D., a gardener, aged 41. Duration of diabetes uncertain. An ophthalmologist diagnosed diabetes three months before admission, while the sight of the right eye had begun to fail twelve months before that. Admitted March 18th, 1924, on a diet of 1,524 calories ("B 15": carbohydrate 78 grams, protein 60 grams, fat 108 grams. G : FA : : 1 : 1). On March 19th the urine was acetone-free and the total glucose lost in twenty-four hours was 23.4 grams. The diet was gradually and with difficulty built up to 1,505 calories ("C 15": carbohydrate 50 grams, protein 49.5 grams, fat 123 grams. G : FA : : 1 : 1.6). Blood examinations were made during April 10th; these were repeated on April 11th, 100 grams of intarvin having been given in addition to the breakfast. The intarvin was continued at the rate of 100 grams per diem on the same diet until April 16th. The figures for April 17th represent the termination of the intarvin period. The specimens recorded as before 10 a.m. are fasting samples. Details of examinations in this case are given in Table III.

TABLE III.—CASE II.

(All chemical data are given in milligrams per cent.)

Date and Time.	Blood.			R.Q.	Urine.	
	Sugar.	Total Fat.	Acetone.		Sugar.	Acetone.
April 10—						
9.30 a.m.	222	809	12.8	0.79	—	8.9
10 a.m., breakfast.						
12 noon	222	835	10.6	0.80	—	—
4.30 p.m.	200	894	14.8	9.92	0.5%	26.7
*April 11—						
9.5 a.m.	200	813	10.6	0.91	—	29.4
12 noon	232	913	10.6	0.73	—	—
4.30 p.m.	221	769	10.6	0.75	—	46.0
*April 12—9.30 a.m. ...	265	1114	17.0	—	—	46.7
*April 13—9.30 a.m. ...	265	1110	12.8	—	—	28.2
*April 14—9.30 a.m. ...	210	1072	27.7	0.91	—	20.2
*April 17—9 a.m.	205	783	47.4	0.81	—	30.3

* Indicates days during which intarvin was given.

The twenty-four hour urine during the intarvin period showed a slight trace of sugar, not estimable. Cholesterol and general metabolism figures revealed nothing. During the period of intarvin administration there was a temporary rise in the fasting blood fat and a gradual rise in the blood acetone percentage, fairly well reflected in the findings for urinary acetone when compared against the fasting blood samples. The respiratory quotient showed no typical change. The faeces during the intarvin period contained 47 per cent. fat, of which 84.5 per cent. was unsplit. The large percentage of unsplit fat was probably due to the unabsorbed mineral oil portion of the intarvin.

CASE III.

D. B., aged 30. Duration of diabetes, fifteen months. On April 19th, 1924, the patient was put on a diet of 1,524 calories ("B 15": carbohydrate 78 grams, protein 60 grams, fat 108 grams. G : FA : : 1 : 1). The urine showed acetone, and 41.1 grams of glucose were lost in the twenty-four hours on this diet. The patient was made sugar- and acetone-free with difficulty. The diet was built up to 1,896 calories ("C 19": carbohydrate 61 grams, protein 57.5 grams, fat 158 grams. G : FA : : 1 : 1.5). With the diet at this level on May 20th, from May 21st to May 29th the same diet was given, with, in addition, 100 grams of intarvin per diem. During this period the blood acetone was only slightly increased, and the total fats and cholesterol remained fairly steady except towards the end, when the blood fat started to rise. The cause of this cannot be explained. Later, when the patient

TABLE IV.—CASE IV.

Date and Time.	Diet.				Blood.			R.Q.	Morning Urine.		24 Hours' Urine.	
	C.	P.	F.	Intarvin.	Sugar.	Total Fat.	Acetone.		Sugar.	Acetone.	Sugar.	Acetone.
A. *Intarvin and Pre-stabilization Period.*	gm.	gm.	gm.	gm.	mg. %	mg. %	mg. %		mg. %	mg. %	gm.	gm.
July 16, 1924—												
10.5 a.m.	25.0	37.75	50.5	0	125	—	16	—				
12.12 p.m.					118	861	87	—				
4.15 p.m.					114	1066	—	0.88				0.15
July 17—												
9.30 a.m.	31.5	39.75	72.0	100	114	926	52	0.77				
12.15 p.m.					111	935	215	0.77				
1.35 p.m.					—	932	169	—				
4.0 p.m.					138	957	61	0.77				0.69
B. *Intarvin added to Stable Diet.*												
August 6.—9 a.m.	81.0	69.5	200.0	100	—	1054	11	0.73	—	9	—	—
,, 7.—9 a.m.	81.0	69.5	200.0	100	125	1047	6	0.70	—	15	Sl. trace	—
,, 8.—9 a.m.	81.0	69.5	200.0	100	108	1127	8	0.73	—	—	Trace	—
,, 9.—9 a.m.	81.0	69.5	200.0	100	104	1235	64	0.74	—	11	Trace	0.15
,, 10.—9 a.m.	36.0	29.0	77.0	100	—	—	—	—	—	—	Trace	—
,, 11.—9 a.m.	77.6	69.0	190.5	100	105	1264	75	—	—	27	Trace	0.87
,, 12.—9 a.m.	77.6	69.0	193.5	100	110	871	45	0.78	—	16	Trace	0.71
C. *Fat added to Stable Diet.*												
August 14.—9 a.m.	77.6	69.0	240.5	*	100	1034	83	0.75	—	—	—	—
,, 15.—9 a.m.	77.6	69.0	240.5	*	111	1052	149	0.76	—	107	—	—
,, 16.—9 a.m.	81.0	69.5	200.0	*	91	956	57	0.70	—	169	Sl. trace	—
,, 21.—9 a.m.	77.6	69.0	273.5	†	—	—	—	—	—	—	—	—
,, 22.—9 a.m.	77.6	69.0	190.5		97	893	86	0.66	—	151	—	1.5

Note.—In sections B and C above the figures are obtained from samples of blood taken, and of urine passed about 9 o'clock each morning while the patient was still fasting.

* In section C 50 grams of butter fat replaced the intarvin previously given. † 100 grams olive oil added to diet of August 21st.

was put on insulin 20 units per diem, the fats rose still higher. The total urinary acetone for twenty-four hours was on May 21st 0.35 gram, and on May 28th 1.35 grams. There was a faint indeterminate trace of lower sugar per twenty-four hours during most of the intarvin period. During the whole of the period the fasting blood sugar was below the normal threshold value. Thus, while the evidence obtainable for blood acetone was doubtful, we found that the total acetone excretion had risen. As in Case II, the faeces showed a high percentage of unsplit fat during intarvin administration, probably for the same reason.

CASE IV.

D. R., aged 55. Diabetes discovered four years previously during examination for life insurance. Apparently the disease had always been of the mild type. The patient was admitted on July 10th, 1924. After a test meal of 50 grams of glucose the blood sugar rose to 200 mg. per cent. at the end of the first hour, but had returned to normal at the end of three hours. In this subject the intarvin was administered in two different periods: (*a*) in the pre-stabilization period as the diet was being built up (July 16th and 17th); (*b*) from August 6th, when the diet was in a more stable condition. During the first period it was found that the addition of intarvin to the diet was associated with increase of acetone.

The diet was then built up without intarvin (to "C 25"—that is, 2,520 calories per diem), but to make sure that even with intarvin the diet would still be within the patient's tolerance it was kept 100 calories lower during the experiment—that is, 100 grams of intarvin were added to a diet containing 2,400 calories from August 6th to 12th. August 10th, however, was made a low day, with only 952 calories besides the 100 grams of intarvin, and the diet on August 11th and 12th was kept at 2,300 calories along with the intarvin. With the exception of the first day, the twenty-four hours sample of urine showed an indeterminate trace of sugar throughout the whole period. This disappeared on stopping the intarvin. The diet was kept at 2,300 calories per diem during August 14th and 15th, and 50 grams of butter fat were added each day. On August 21st 100 grams of olive oil were given along with ordinary diet, and acetonuria was definitely produced.

It will be noticed that, even during the stabilized period, intarvin when added to the diet tended to increase the percentage of acetone in the blood and also the total excretion of acetone, especially towards the end of the period. This, in view of its supposed antiketogenic function, is rather disappointing. The increase, however, is not so marked as when ordinary fat is given—for example, the figures of August 14th and 15th, when 50 grams of fat were given to replace the intarvin. When 100 grams of olive oil were taken on August 21st there is again a much greater increase in the total acetone lost.

Discussion.

From the above observations on the use of intarvin in diabetes it would seem that further investigations are needed into the whole question before the claims on behalf of this fat can be fully substantiated. In the cases recorded no reduction, but rather an increase, in the amount of acetone in the blood followed the administration of intarvin. The rise, however, is less than when an equivalent quantity of other fat is added to the diet. The average G : FA ratio in the diets employed was 1 to 1.5, so that there was ample carbohydrate to burn up the ordinary fat of the diet, apart from the intarvin. The respiratory quotient did not at any time give indication of more carbohydrate than usual being burned under the influence of intarvin, as might be expected if the acid ultimately broke up to form sugar. On the other hand, the addition of intarvin to a stabilized diet always led to the excretion of small traces of sugar in the urine. The changes in the blood fat under intarvin are simply recorded. These cannot be explained as yet. Further investigation of this question is in progress. When a patient's diet has been stabilized at his maximum tolerance, 100 grams of intarvin can be added (allowing about 700 extra calories) without the appearance of clinical signs of acidosis. But, in view of the conflicting figures obtained regarding the control of ketosis consequent on its use, intarvin cannot be used indiscriminately to build up diets to a maintenance level without fear of ketosis. The expense

and the unpalatability of the substance will also militate against its general acceptance in practical dietetics.

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THE PERMEABILITY OF MENINGES TO URANIN AS A METHOD IN THE DIAGNOSIS OF MENINGITIS.

(Preliminary Communication.)

BY

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A METHOD whereby it is possible to decide simply by lumbar puncture whether meningitis is present or not may possibly be of interest. The normal meninges and plexus choroides have the power of preventing a number of substances that are foreign to the organism from passing into the spinal fluid; diseased meninges have lost this power. Especially in meningitis this permeability of the meninges to chemical substances is greatly increased, and it is thus natural to investigate whether the determination of the latter can be of assistance in diagnostic difficulties. The substance must be easily found in the spinal fluid and be non-poisonous. Such a substance is uranin (sodium fluorescein), a red colouring matter very readily soluble in water; a weak solution gives an intensely light-greenish fluorescence which strongly recalls the fluorescence seen in a positive Schlesinger reaction. The fluorescence does not disappear until the solution is weakened to 1 in 10,000,000. Weaker solutions have a dirty greenish colour, without fluorescence.

A series of investigations carried out by Kafka,[1] Schönfeld,[2] Jörgensen,[3] and others have established that uranin, in doses of 6 to 8 grams by the mouth, passes into the spinal fluid in about 30 per cent. of healthy individuals, but in small quantities only. In dementia paralytica, and also in syphilitic persons, uranin was found more frequently in the spinal fluid, especially if it was altered. These authors do not appear to have made any determination of the permeability of the meninges in meningitis.

Uranin given in larger doses enters the spinal fluid where the meninges are not infected, and the patient's skin becomes intensely yellow. After several experiments we decided upon 2 grams of uranin for an adult, or about 0.03 per kilogram of body weight. One or two hours after such a dose the patient turns yellow, but the coloration is not stronger than in icterus, and the yellow colour of the skin and mucous membrane disappears comparatively quickly, as a rule after five to six hours. Uranin has no poisonous effects, and according to C. Jörgensen's investigations does not irritate the meninges. It can be administered by the mouth or by intramuscular injection in a 20 per cent. solution, but not subcutaneously, as a strong solution produces a sensitive infiltrate. After half an hour to an hour the blood serum gives a greenish fluorescence, which in the great majority of cases does not disappear until the serum is weakened to 1 in 64. The concentration of uranin in the blood is strongest two to three hours after it has been given, and it is at that time, therefore, that the lumbar puncture should be performed. We employ the following technique:

The patient is given 2 grams of uranin by the mouth (preferably in wafers) or by intramuscular injection (20 per cent. solution) if he is not quite conscious. Three hours later lumbar puncture is undertaken in the usual manner. In order to ascertain whether the spinal fluid is fluorescent or not, the test tube must be held against a dark background. In meningitis the fluid gives off a distinct greenish fluorescence. No importance should be attached to a doubtful fluorescence. The presence of blood may confuse the reaction, since the blood contains great quantities of uranin. Investigations made at this section prove, however, that at least 1/3 c.cm. of blood must be mixed with 10 c.cm. of spinal fluid in order that the latter shall become fluorescent.

We have employed this simple method with 74 patients, 18 of whom were suffering from various forms of meningitis. The spinal fluid (from, in all, 22 lumbar punctures) taken from patients suffering from meningitis was in all cases fluorescent, in some quite strongly, like a markedly positive Schlesinger reaction. It is true that the fluorescence was weak in three instances, but nevertheless distinct. The method used in these cases, however, differed somewhat from the technique usually employed, the lumbar puncture being performed within one hour after the patient had received the uranin. In addition, the dose of uranin given to two of the patients was too small (1 gram). The spinal fluid in 38 of 56 non-meningitic cases was as clear as water. In 8 cases the fluid was of a dirty green colour without fluorescence; this shows that minimum quantities of uranin had entered the spinal fluid. In 10 cases the spinal fluid was faintly fluorescent, but in 7 of these there was an admixture of blood, which, as mentioned above, makes the results uncertain. Three of the non-meningitic cases, on the other hand, exhibited a faintly fluorescent spinal fluid three hours after 2 grams of uranin. The fluorescence disappeared, however, in a dilution of 1 in 2, whilst the spinal fluid from patients with meningitis showed fluorescence in a dilution of 1 in 8 and up to 1 in 30. (For this determination we employed small test tubes 11 mm. in width, and observed the fluorescence in a dark box, where the light at a distance of 15 cm. enters through a 20 dioptric biconvex lens.)

Conclusions.

Uranin in doses of 0.03 gram per kilo of body weight does not enter the cerebro-spinal fluid, or does so only in small quantities if the meninges are not infected. In meningitis the colouring matter enters the fluid in easily demonstrable quantities. If, therefore, by lumbar puncture three hours after uranin has been given in the doses stated, the spinal fluid shows a distinct greenish fluorescence, we can with great probability diagnose meningitis without making other investigations of the spinal fluid. If the spinal fluid shows no fluorescence, meningitis can with certainty be excluded.

It is not possible to decide what form of meningitis, if any, is present. The passing of the colouring matter into the spinal fluid merely indicates the greater or lesser permeability of the meninges. In meningitis comitans, circumscripta, etc., the uranin does not enter the fluid, or does so in relatively small quantities, even when pleocytosis is present. Opinions differ as to whether it is correct to designate these conditions meningitis, but this is doubtless chiefly a dispute about words.

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A WEAK POINT IN STERILIZING METHODS.*

BY

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ONE of the problems of the operating theatre is to ascertain the temperature reached in the centre of a packet, tin, or drum of dressings in a sterilizer. It is very different from that of the steam around the outer side of the packet or tin, but the latter alone is registered by a thermometer projecting into the sterilizer and capable of being read from the outside, or, more commonly, by means of a pressure gauge. I made tests with a specially prepared thermometer, similar to a clinical thermometer

* Read at a meeting of the Malaya Branch of the British Medical Association.

but registering far higher temperatures. I placed it inside a tin of dressings, which I then "sterilized" in a high-pressure sterilizer of the largest and most up-to-date pattern. When the temperature of the steam in the inner jacket of the sterilizer was 240° F. that in the middle of the tin of dressings was only about 160° F. After screwing down the safety valve of the sterilizer and "sterilizing" for more than an hour after the maximum pressure had been registered by the gauge, the special thermometer in the centre of the tin registered 220° to 230° F. I tested many other sterilizers, and found that all those of low pressure, such as are used ordinarily in nursing homes and small hospitals, were practically useless, whilst even those of high pressure were mostly inefficient unless "sterilization" was continued for a far longer period and at a higher pressure than that suggested in the maker's printed instructions. On one occasion a theatre sister, doubting the accuracy of my suggestion that her high-pressure sterilizer was not efficient, took the trouble to place a raw egg in the middle of each packet of dressings, but, after completing the "sterilizing" process, discovered that the eggs were still raw. Afterwards she "sterilized" several times at a temperature and pressure regarded as dangerously high, previously having placed my special high-registering thermometer in the inside of the drums of dressings. She discovered that my thermometer even then rarely registered a temperature exceeding 200° F.

It is possible, of course, to argue that if no mischief has been proved to have resulted from the ordinary methods of "sterilization" it matters little whether or not a very high temperature has been obtained in the middle of the tin of dressings. It would be equally possible, however, to argue that, if no mischief has resulted from the use of dressings that have not been "sterilized" at all, "sterilization" is a superfluous precaution. I scarcely believe, nevertheless, that failures to detect and record mischievous results are likely to convince competent surgeons that "sterilization" by sterilizers is useless. Probably they will continue to believe that, when "cooking" our dressings, towels, overalls, etc., we ought to aim at obtaining in the inside of each drum or packet a temperature at least equal to that of the boiling point of water (212° F.).

I have for many years past endeavoured to solve the problem of ascertaining easily and economically the temperature inside a tin of dressings undergoing "sterilization." There is no difficulty, of course, in ascertaining this temperature by means of the special thermometer already mentioned. Such a thermometer, however, is unsuitable for ordinary use, being costly, easily broken, and inconvenient. I have tried, therefore, to find an economical and convenient substitute.

On discovering that a famous surgeon had been experimenting with tiny tubes of phthalic acid, I obtained a number of these from the Paris firm that made them. I placed a tube within each of several tins of dressings, which I then proceeded to "sterilize." The powder in each tube was supposed to change colour from white to red as soon as a certain very high temperature (I think 300° F., but am not quite sure) had been reached. I found, however, by experiments, that the powder always changed colour from white to red during "sterilization" of a tin, no matter what temperature was indicated as having been reached. I found also that the powder changed colour to red when I left a tube in my waistcoat pocket for a week, or even when I held it in a warm hand for a time. Clearly, therefore, these tubes of phthalic acid do not suffice for our present purposes.

Afterwards I gave a trial to various kinds of wax. Finally, I made a series of experiments with fusible metals, and was encouraged, eventually, to devise the pyrometer here described. It is placed in the inside of a tin, drum, or packet of dressings, etc., and remains there during the process of "sterilization." It consists of a round piece of cardboard, to which are attached six small squares of fusible metal, each having a different fusing point. Convenient fusing points have been found to be 190°, 200°, 212°, 220°, 230°, and 240° F. Each square is marked with its own fusing point, and the cardboard is similarly marked in the proper places. When the temperature of 190° F. is reached the first square fuses; when that of 200° F. is reached the second square fuses, and so on. Consequently, by glancing at the pyrometer when the dressings are unpacked after "sterilization," the surgeon can see how many squares have fused, and consequently what minimum temperature has been reached in the inside of the packet or tin. When a square fuses its surface becomes rough and its outline blurred, whereas an unfused square remains bright and its edges sharp. As the pyrometer can only be used once cheapness is essential. A pyrometer consisting of only one thin square of fusible metal melting at exactly 212° F. would suffice, and its cost would be insignificant.

Messrs. Matthey and Johnson of Hatton Garden make these metals to melt at any temperature desired. They all contain bismuth, and this renders the metal somewhat brittle to roll into sheets, but it can be cast fairly easily into thin plates or melted into tiny square castings. The accuracy of the actual melting points is easily checked.

It has rather surprised me that none of the surgical instrument makers, apparently, has ever seriously endeavoured to provide surgeons with a check or reliable test of the machines they supply, which are believed and claimed to sterilize the contents of the largest drums, even when tightly packed. I noted that the standard type of dressing drum, with holes that close and open, possesses no advantage over an ordinary plain metal box with a hinged lid, which is closed over a layer of cotton-wool. The superheated steam appears to penetrate into the latter, even when closed, just as readily as into the former, and the latter has the advantage that it more nearly approaches the ideal organism-proof receptacle than the former, with its numerous loopholes for the possible entrance of bacteria.

A DIATHERMY SNARE FOR REMOVAL OF POST-NASAL GROWTHS.

BY

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THE diathermy snare here illustrated was devised by me and I have used it on an angio-fibroma of the naso-pharynx with success.

The snare consists of a vulcanite body, with metal liner with solid end, into which the diathermy cable screws. In the liner of the body of the snare is a hollow metal slide. The wire, which is of hard drawn silver, is passed through the stem of the snare into this slide, and a vulcanite-headed screw works through a slot in the body into the slide and grips the wire in it. The diagram, in which the snare looks rather frailer than it actually is, shows the mechanism clearly. The instrument was made for me by Messrs. Watson (Kingsway), and the arrangement of the instrument is their design.

The case in which I used it was that of a boy of 13 with an angio-fibroma springing from the basi-sphenoid by a pedicle half an inch broad and a quarter of an inch thick. The tumour was about 1¼ in. long by 1 by 3/4 in., and showed below the soft palate. The wire was passed through the mouth over the pedicle and forced against the basi-sphenoid with the finger-nail; it was then tightened and the current was switched on at a fairly high amperage. The wire came home and the tumour fell off in about three seconds. There was no stain of blood. At no time after the operation did the child have any pain at all. Five weeks later I saw the boy and was unable to detect any sign of growth. Apart from the simplicity of the procedure and freedom from haemorrhage with the diathermy snare, it seems probable that there will be less liability to recurrence after the use of the diathermy snare than if the tumour had been removed by ordinary surgical procedures.

The only precaution is to see that the child's nasopharynx is low, as the tumour falls free and might be aspirated into the glottis.

Microscopic examination showed that the tumour in this case was very vascular.

A TINEA X-RAY COUCH.

BY

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IN *x*-ray treatment of tinea tonsurans irradiation of the Kienböck-Adamson suboccipital area, and to a less degree the vertical one, often gives some difficulty owing to variation in the shape of heads, and also from the risk of pressure on the nose—and consequent movement of the head—where the prone position on the couch is practised. The alternative position of sitting on a chair with forehead supported on the dorsa of the hands resting upon the couch is oftener than not accompanied by a tendency to slip down somewhat in the chair and so disturb the position of the head. To obviate these contingencies and at the same time retain the prone position, which ensures stability better than any other, I designed a couch which was made by the master of works, County of Dumbarton Education Authority, for the skin clinic, where it has been used since March 17th, 1923. It is constructed as follows. In the top of the couch is introduced a movable flap with circular aperture to accommodate the face, and so hinged as to allow of its being easily raised or lowered, at the free end, in relation to the level of the top of the couch. A screw, technically known as a "Leggott's quadrant," which is operated with a short "endless" cord, is attached at the free end of the flap. The cord passes over two small pulleys attached to the under surface of the head of the couch. The flap remains firmly fixed in the desired position without having to wind the cord round any part of the couch; it can be altered at any moment by manipulating the cord, and is capable of the nicest adjustment. The accompanying photograph of the under surface of the top of the couch illustrates the mechanism. It will be seen that all pressure on the nose is avoided and variation in cranial shape combated by the hinge adjustment. Experience of working with it has suggested constructional modifications which would to some extent increase its efficiency. One is to change the position of the flap so that its free edge would form part of the end of the couch; another is slightly to alter the shape of the aperture, or, better still, to make it variable; these modifications I am about to introduce.

Under surface of tinea couch, showing method of operating hinged flap.

Memoranda:

MEDICAL, SURGICAL, OBSTETRICAL.

PULMONARY EMBOLISM.

IN view of recent discussions on pulmonary embolism and thrombosis, the following case may prove of interest.

Mrs. H., aged 55, was admitted to the Christie Hospital with epithelioma of the vulva. She was treated with radium, and, ten days later, by diathermy. Her general condition, both before and after operation, was good. Her temperature was raised for three days subsequent to the operation, the maximum being 100° F., and she complained of constant pain at the site of the lesion for the first week or ten days. She felt perfectly well, but was unable to sit up, owing to the tenderness of the raw surface left after the diathermic treatment.

Three weeks after operation she was still in bed and the wound was about two-thirds healed. On the morning of the twenty-second day she complained of high epigastric pain and some dyspnoea. She was noticed to be slightly cyanosed, and the pulse and respiration rates were increased. These both rose rapidly, the cyanosis deepened, the pulse became weak and thready, and collapse supervened. Ten minutes after the onset of symptoms she lost consciousness, the breathing became stertorous, the pulse and respiration rates dropped, and death took place about five minutes later.

Post-mortem Report (Dr. Powell White).—The pulmonary artery contained blood clot which was mostly recent and appeared to have been formed immediately before death, but some was of earlier date; it was not adherent. The clot extended to the smallest arterial branches in the upper lobes of both lungs, but not into the lower lobes. The upper lobes were partially collapsed and dry, while the lower ones were normal and moist. The right ventricle and auricle also contained recent blood clot. No distinct embolus could be found. The blood in the left side of the heart, the aorta, the venae cavae, the portal and the iliac veins was quite fluid. The smaller pelvic veins, especially those coming from the uterus and vagina, were distended with blood clot. The external iliac veins on both sides were enlarged and hard.

This case was in all probability one of pulmonary embolism followed by thrombosis in the pulmonary artery; it is probable that the embolus came from one of the pelvic veins in the neighbourhood of the uterus or vagina. An illustration is provided of one certain and another possible etiological factor in pulmonary embolism. The main factor here must have been the patient's enforced recumbency for three weeks; she had lain quite still all day and night, in spite of being urged to move about as much as possible without causing herself undue pain. The circulation in the veins of the pelvis and lower limbs must have been at an extremely low ebb, especially as the force of her heart beat, though adequate, was never great. The other factor which probably played a part was sepsis. The embolus evidently arose from one of the smaller uterine or vaginal veins, these having been found at the necropsy to be distended with clot. The venous drainage of the site of the necessarily septic lesion, the perineum, is ultimately the same as that of the uterus and vagina—namely, into the internal ilias veins. It is also extremely likely that infection had spread directly from the perineum along the vagina into the parametric tissue, which contains those veins that were found to be affected.

Manchester. E. S. FRISCHMANN, M.B., Ch.B.

BILATERAL CYSTIC EMBRYOMA OF OVARY WITH TORSION OF THE PEDICLE OF ONE CYST.

THE following case seems to be of interest on account of the fact that a cystic embryoma existed in each ovary and that there were no definite symptoms until the torsion of the pedicle of one took place.

A married woman, aged 36, sent for me on the evening of November 13th, 1924, complaining of pain in the abdomen and difficulty in passing water. She had been treated for floating kidneys with a Curtis belt. One child, born ten years ago, died at 4 years of age. Her periods were regular and normal.

History of Present Condition.

She was quite well until the morning of November 13th, when she awoke with pain in the lower part of the abdomen and difficulty in passing urine. She stated that she had had similar attacks, but had put them down to her floating kidneys. A period was then just ending.

Condition on Examination.

When I saw her she was sitting up in bed and leaning forward, as this position gave her relief. The temperature and pulse were normal. She was of spare build and had a goitre with some exophthalmos.

The abdomen showed slight bulging above the symphysis pubis, which on palpation gave the impression of a cystic tumour rising from the pelvis in the mid-line halfway to the umbilicus. On

pressing on the tumour she felt a desire to micturate. The right kidney was palpable and the left distinctly mobile. The uterus was retroflexed and separate from the swelling, which lay between it and the symphysis pubis, and felt like a tense full bladder. Nothing else abnormal was found in the pelvis.

There being no facilities for a hot bath, etc., I obtained the services of a nurse to pass a catheter. She reported that she withdrew only 2 drachms of urine. When I again examined the patient there was no change in the condition. I saw her the next morning, and although she had passed urine in small quantities the tumour persisted.

She was admitted to the Kingston Victoria Hospital, where she first had a hot bath. I then passed a metal catheter. The bladder was empty and the beak of the catheter could be felt in the stretched-out bladder superficial to the tumour. Ovarian cyst, probably on account of its position a cystic embryoma, was diagnosed. The temperature was 99° and the pulse 76. As there appeared to be no urgency, operation was not performed until the following day, November 15th.

Operation.

The usual subumbilical incision was made and a greatly engorged ovarian cyst presented. This was delivered intact through the wound and found to arise in the right ovary. It had rotated on its pedicle through two complete turns. The pedicle was clamped and divided, and the stump ligatured and covered with peritoneum. The left ovary contained numerous follicular cysts and one larger one the size of a bantam's egg. The latter was incised and a creamy fluid, followed by hair, exuded. The opening was clamped and the cyst dissected out. The rest of the ovary was left, as it evidently contained a considerable amount of active ovarian tissue. The abdomen was closed in layers. The Fallopian tube was spread over the surface of the larger cyst. It was as large as a full-time foetal head. On incision a quantity of cream-like sebaceous material, with a lump of hair, escaped. The inner wall was smooth, except for one spot, from which a tuft of hair was springing. The smaller cyst showed, projecting from its inner wall, a prominence the shape of a rabbit's heart, from which hair was growing. This prominence had a central cavity apparently lined with mucous membrane. A microscopical section showed a cystic embryoma with great hyperplasia of the sebaceous glands. Sweat glands and hair follicles were also seen, and externally a layer of tissue resembling lung.

There is little doubt that the larger cyst used to become partly twisted on its pedicle, which would account for her previous slight attacks. But even though the torsion was found at the operation to be complete and the cyst and Fallopian tube almost gangrenous, there were none of the usual symptoms and signs of an acute abdomen.

Kingston-on-Thames. L. J. FORMAN BULL, M.B., B.S.Lond.

Reports of Societies.

PERFORATION OF GASTRIC AND DUODENAL ULCERS.

A HUNTERIAN Lecture was delivered at the Mansion House on January 19th by Mr. G. GREY TURNER (Newcastle-upon-Tyne), who took for his subject "The perforation of gastric and duodenal ulcers." Mr. H. W. CARSON, President of the Hunterian Society, was in the chair.

Mr. Grey Turner, whose lecture was accompanied by many tables of statistics, said that since 1901 he had had to do with 194 cases of perforation, but these included some cases of localized perforation, incomplete operations, and cases in which it was eventually decided not to operate. Eliminating these, and confining himself to general perforations in which he performed a complete operation, he was left with 147 cases. These cases had resulted in 124 recoveries and 23 deaths, the percentage of deaths being 15.64. But if only those cases were taken which had been operated on within twelve hours of perforation, the death rate was reduced to 8.16 per cent. The actual figures were: perforated gastric ulcers operated upon within twelve hours, 27, with 3 deaths; perforated duodenal ulcers, 71, with 5 deaths. Thus a very essential point in the treatment of this condition was early diagnosis. If a case were taken in hand reasonably early and carried to a successful issue, the credit lay with the general practitioner who had detected the condition and advised the operation, rather than with the surgeon who had actually carried it out. He was sure that there was no new refinement of operative technique which had anything like the value of a reduction in the period elapsing between perforation and operative interference. Of his 194 cases, 147 were males and 47 were females. With regard to age, it had rather surprised him to find how many of the patients were over 50—namely, 6 gastric cases and 40 duodenal. The fact, therefore, that symptoms occurred in a comparatively elderly person need not deter the making of such a diagnosis. He wished also to emphasize the frequency of acuteness in the ulcer which perforated. The perforation which occurred was often that of a second contact or "kissing" ulcer on the opposite wall of the stomach to that of the chronic ulcer. He was convinced that there was an acute ulcer whose principal characteristic it was to perforate. With regard to the after-history of the 120 recovered cases which could be traced, 61 were quite well, 19 had slight digestive trouble, 18 had had to undergo a further operation, 2 had recurrent trouble and required further operation, 8 had died from some sequel of the original trouble and 9 from other causes without recurrence, and 3 were known to be well shortly after the operation and were then lost sight of. With regard to the choice of surgical procedure, he thought that the first duty of the surgeon was to save the life of the patient, and in this connexion his figures with regard to immediate gastro-enterostomy suggested that this resort added to the mortality. He had done primary gastro-enterostomy in 26 cases, with 20 recoveries and 6 deaths, and of the 98 cases without primary gastro-enterostomy 81 had been recoveries and there were 17 deaths. He was of opinion that the best thing to do was to get the patient over the calamity of perforation, and then, if it was evident that a gastro-enterostomy was necessary for the cure of the trouble, to perform this as a delayed operation before the patient left the hospital. He had done such a delayed operation in 11 cases, and there had been no deaths. The average hours of perforation in these cases had been nine, and the longest interval between perforation and gastro-enterostomy twenty-eight days, the shortest interval twelve days. He spoke with enthusiasm of the value, in bad cases of general perforation, of a temporary gastrostomy. He had employed this measure 28 times, and there were 8 deaths, but, although this death rate of 30 per cent. was a high one, it had to be remembered that the average number of hours of perforation in these cases was seventeen. In 22 operations for localized perforation also gastrostomy was used twice, and both patients died. Nevertheless, he felt that this procedure in many cases had been of the greatest possible help. With regard to irrigation of the peritoneum, he had lost his early belief in this procedure, and he exhibited a table showing that in 117 cases irrigated there had been 19 deaths, and in 25 cases not irrigated 4 deaths; but while the percentage of deaths was much the same in both these categories, the average period of perforation in the cases irrigated was eleven and a half hours, and in the non-irrigated cases twenty-two hours. Also, out of 23 cases which developed gross complications, 20 had been irrigated—an incidence of 17 per cent. of irrigated cases. In operating he thought the surgeon should be content with the middle line of incision. Very often the stomach was distended, and in these circumstances great help would be found in passing a stomach tube while on the operating table, when the stomach would go down like a big balloon. With regard to complications, subdiaphragmatic abscess had occurred in 8 cases with 3 deaths, pneumonia and pleurisy in 7 cases with 2 deaths, pelvic abscess in 3 cases with 2 deaths, gastric fistula in 3 cases with 1 death, pulmonary embolism in 1 case which died, and prolapse of intestine in 1 case which recovered.

Mr. JAMES BERRY complimented Mr. Grey Turner on the way in which he had trained general practitioners in Newcastle to recognize gastric and duodenal perforations and send their patients without loss of time to the infirmary. He was of opinion that in the great majority of cases there was no need to drain at all; in a certain number it was well to put in a single drain, but it should be taken out next day. He also believed that irrigation should rarely be done. His own practice had been not to do gastro-enterostomy, which seemed to add a great deal to the risk of the operation.

Mr. JAMES SHERREN said that twenty years ago the acute ulcer which perforated was met with much more frequently than now; the majority of cases which perforated nowadays seemed to be chronic, and to have given symptoms for many years. If the ulcer, whether of the stomach or the

duodenum, was a chronic ulcer, he thought that, in addition to closing the perforation, at some time a curative operation must be done. The choice of the time would depend on the experience of the surgeon and the condition of the patient.

Mr. R. P. ROWLANDS thought that if the operation of gastro-enterostomy became a routine a high and quite unjustifiable mortality would result; but in a few cases in which it was not possible to close the perforation a gastro-enterostomy might be the only life-saving measure.

Mr. GORDON-TAYLOR agreed that the mortality of perforation of gastric and duodenal ulcer had been lowered, not by any particular modification of surgical technique, but by the comparatively early date at which, even in London, cases of perforation now came into the hands of the surgeon.

Mr. A. J. WALTON said that in his experience these ulcers were nearly always chronic. This was borne out by the history elicited in a very large proportion of the cases which had come under his observation, by the indurated edge of the ulcer, and by the findings at necropsy. He thought that in nearly every one of these perforated ulcers gastro-enterostomy should be done, but, he agreed, not always at the time of the operation.

Mr. T. P. LEGG was of opinion that, as a general rule, they should be content with suturing the ulcer without doing a gastro-enterostomy. Mr. W. E. TANNER referred to the wide distance it was necessary to go with the sutures beyond the perforation to get them to hold; this bore out the idea of chronicity.

Mr. GREY TURNER, in replying on the discussion, said that in certain cases gastro-enterostomy should undoubtedly be carried out at the primary operation, but he thought that the table of mortality would be lowered if it was generally agreed that whenever possible the operation should be left for a later occasion.

PNEUMOCOCCAL PERITONITIS.

A JOINT meeting of the Sections of Children's Diseases and Surgery of the Royal Society of Medicine was held on January 23rd, with Dr. HUGH THURSFIELD in the chair.

Dr. HECTOR C. CAMERON introduced a discussion on pneumcoccal peritonitis. While the incidence of this disease fell very largely amongst girls of 5 to 15 years of age, it was overstating the case to say, as some authorities had said, that it was confined to the female sex, since he had seen three cases amongst boys. In early infancy this disproportion was much less marked, and the two sexes were almost equally, though comparatively rarely, attacked. As regards symptoms, there was often a prodromal period of some days, during which diarrhoea was very frequently present before severe abdominal pain and a rise in temperature announced the spread of infection to the peritoneum. This prodromal diarrhoea, though not invariably present, was a very valuable aid to diagnosis; two illustrative cases were cited. Primary pneumococcal peritonitis (the secondary form hardly came within the scope of the discussion, since the peritonitis was but a small part of generalized pneumococcal infection) had been divided into two types—a fulminating and a comparatively mild. Since it was possible to have every gradation between these two extremes, Dr. Cameron preferred to consider the disease rather as one exhibiting three phases—a stage of onset, a stage of high pneumococcal fever, and a stage in which the outstanding symptom was a residual collection of pus in the peritoneum (an "abdominal empyema"). The onset was generally sudden and marked by severe abdominal pain. Within three hours in one case a child exhibited delirium, with a temperature of 105° F. The patient rapidly became flushed and drowsy, and the abdomen distended. This child died a few days later after laparotomy had been performed. The dramatic suddenness of onset in such cases resembled that of perforation of a gastric or duodenal ulcer, with which they were happily unlikely to be confused owing to the rarity of the latter condition in childhood. Less severe cases, however, might resemble appendicitis, and were frequently diagnosed as such. If life was prolonged beyond this stage there followed a period of high fever, lasting two or three weeks or longer, during which there was comparatively little pain and few signs of abdominal disturbance. Dr. Cameron believed that this coincided with the exudation of fluid into the abdomen. During this phase the differential diagnosis had to be made from typhoid fever. Irregular intermission of the fever and high pulse rate heralded the third phase, during which the patient began to be sensible of his surroundings. Finally, a localized abscess or abscesses formed, and recovery usually followed drainage. The abscess was generally in the pelvis or the subphrenic region, but was occasionally divided up into loculi spread irregularly over the abdomen. In one case six separate abscesses had formed at long intervals. In this stage the differential diagnosis lay between pneumococcal and tuberculous peritonitis, as the child was wasted and ill, and presented a distended abdomen with signs suggesting free fluid. Rarely there were latent forms in which the abscess had a tendency to evacuate itself through the bowel or bladder. In other cases the resemblance to tuberculous peritonitis was heightened by the pointing of the abscess at the umbilicus, but with the difference that the pneumococcal abscess burst with great rapidity, whereas the tuberculous abscess, as was well known, might appear to be on the point of evacuating for a long time before doing so. Dr. Cameron added that while there were occasional cases which ran an unusual course this description would fit the majority of cases. The treatment of cases of pneumococcal peritonitis in the "abdominal empyema" stage was obvious—such cases should be drained, and almost invariably did well. But the question of operation during the early stage was not an easy one to answer, and resolved itself into the question of whether or no drainage helped the patient in any way in antibody production. His own experience was that a fall in temperature only rarely occurred after drainage in the acute stage, and that further operation for residual abscess was generally called for. He had therefore come to the conclusion that it was better not to operate until there was a definite localized abscess, but to keep up the patient's strength until that time by every possible means. He applied ice to the abdomen and exhibited opium, and in some cases antipneumococcal serum. Fraser had had considerable success with blood transfusion in these cases, but the speaker had no personal experience of this. This expectant form of treatment must naturally only be employed where the diagnosis of pneumococcal peritonitis was clear; if there was any doubt as to whether the case was one of fulminating appendicitis operation should be performed at once.

Mr. GEORGE WAUGH said that pneumococcal peritonitis belonged to the very small group of diseases which could be diagnosed, at least at a certain stage, with confidence by the clinician without submitting any material to pathological investigation. This form of peritonitis presented the paradox that all the ordinary signs of inflammation of the peritoneum were strikingly and characteristically absent, for within a few hours of the onset pain had disappeared and no tenderness, muscular rigidity, or immobility of the abdominal wall could be elicited. Further, except for slight diarrhoea and vomiting at the outset, the normal functions of the bowel remained unimpaired throughout the course of the illness; even the appetite was good so long as it was not interfered with by excessive pyrexia. The only evidences of persistent inflammation were the swinging temperature and accelerated pulse. This remarkable absence of symptoms had its parallel in the inflammations due to the pneumococcus in all parts other than the lungs and pleurae, for pneumococcal otitis media, pericarditis, arthritis, etc., were all painless within a few hours of the onset. The only positive sign of involvement of the peritoneum was a steadily increasing tumescence of the abdomen first seen in the subcostal grooves, the groins, or the region of the umbilicus. This swelling was due to the outpouring of the green non-irritating pus and not to distension of the gut. As an illustration the case of a baby girl, aged 14 months, was quoted. The child had had a feverish illness of six days' duration which began with sharp but transient pain in the left iliac fossa, with vomiting and diarrhoea for a few hours only. On examination the left side of the abdomen and pelvis, as high as the iliac crest, was found to be full of fluid. As the child had a vaginal discharge he hazarded the conjecture that

the pneumococcal infection diagnosed had spread from the left tube. At the operation the usual large collection of greenish pus was opened, and the left tube, which oozed pus through a patent abdominal ostium, was removed. This child made an uneventful recovery. Another case, in which after operation for empyema the significance of slight abdominal symptoms and tumescence was overlooked, died; *post mortem* the abdomen was found to contain a large quantity of pneumococcal pus, drainage of which would probably have saved the patient. After contrasting the progressive toxaemia and absence of signs of peritonitis which occurred in the pneumococcal form with the striking symptoms and absence of toxaemia which so often characterized tuberculous peritonitis, Mr. Waugh proceeded to the discussion of treatment. Some cases recovered without a diagnosis having been made and without any specific form of treatment. Evidence of this was found in a number of cases who first presented themselves with an encysted abscess in the abdomen. Treatment in the residual stage was standardized and consisted of simple drainage. Treatment during the invasion stage presented the difficulty that the clinical syndrome was sometimes indistinguishable from that of an acute inflammation of an appendix "concealed" beneath the liver, behind the root of the mesentery, or in the pelvis. In such cases immediate operation should be undertaken. When the abdomen was opened the pathological appearances of early pneumococcal peritonitis were unmistakable: a little free fluid, always cloudy and frequently containing a few flakes of lymph, was present, and over a wide area the coils of small intestine were an angry red with their lustre somewhat impaired. Similar changes to an equal or less degree were exhibited by the appendix. In such cases he removed the appendix if involved and drained the abdomen. The latter was essential, for in two cases of early pneumococcal peritonitis in which the appendix had been removed, and the abdomen closed without the condition being recognized, the patient was only saved from disaster by a subsequent reopening of the abdomen and liberation of quantities of characteristic green pus. It was difficult to understand why drainage should be helpful, but it was his impression that it was so. He had never seen a fatal case in which operation was done within thirty hours of onset, whilst many cases of concealed appendix had been discovered. Finally, in the intermediate stage, the need for operation was determined by the degree of toxaemia and its tendency to increase. A climbing pulse rate was an indication for operation, but if the pulse rate remained fairly constant in spite of a swinging temperature, operation might be safely deferred. In some cases the illness came to an abrupt end about the tenth or twelfth day and any fluid in the abdomen was absorbed. Presumably in these cases the pneumococcus was entirely killed, for in the residual cases, which came to operation many months after the original illness, the pneumococcus could always be cultured from the pus. Mr. Waugh considered that the sinister reputation of pneumococcal peritonitis was undeserved; it probably arose from confusion between the primary type of case and pneumococcal septicaemia in which involvement of the peritoneum was but a small part.

Dr. D. N. NABARRO said that the pneumococcus was an organism whose pathogenicity was strikingly variable in different animals. In some it produced a rapidly fatal septicaemia, whilst others were immune. Man occupied an intermediate position, and pneumococcal infections were usually localized. Children, however, were more susceptible than adults, and in them a generalized infection of serous cavities was not very uncommon. Primary pneumococcal peritonitis was, so far as the records of Great Ormond Street Hospital showed, a rare disease. Only fifteen cases had been diagnosed since 1918, and only seven of these had grown pneumococcus from the exudate. Of the fifteen cases diagnosed as pneumococcal peritonitis, five recovered and ten died; all were females. Macartney and Fraser, in their paper on this subject, described the recovery of the same pneumococcus from the pus in the pelvis, the blood, and from the vagina. In the cases at Great Ormond Street seven cases yielded an organism which was morphologically a pneumococcus, but as no bile solubility or pathogenicity tests had been performed, it was possible that the organism was a variety of streptococcus—for example, that sometimes described as the enterococcus. In the case of a female child aged 16 months, diagnosed as pneumococcal peritonitis, the organisms isolated from the pus were *B. coli* and streptococcus; the same organisms were recovered from the vagina. He thought it probable, therefore, that many cases, which clinically were diagnosed as pneumococcal peritonitis, were due to another organism. Macartney and Fraser, in discussing the etiology of this disease, pointed out that its incidence was limited to female children, generally of the poorer classes who were apt to sit about without protection for the perineum, so that they contracted a vulvo-vaginitis. As a result the vaginal secretion was not acid as in health, so that the pneumococcus was able to flourish, and eventually to make its way into the abdomen via the genital tract and tubes. These authors pointed out that the pneumococcus was recoverable from the vagina in every case, that the first symptoms were those of subumbilical peritonitis, and that at laparotomy, in early cases, pus was found in the pelvis only. They had reproduced the disease in a female monkey by introducing the pneumococcus into the vagina after it had been rendered alkaline by washing out with a solution of sodium bicarbonate. The only alternative routes by which the pneumococcus might gain entry into the peritoneal cavity—namely, by the blood stream or through the intestine—were apparently negatived by the fact that all the cases occurred in females. Dr. Nabarro suggested that the milder cases which recovered without operation, and from which in consequence no pathological material was available, were cases of enterococcal and not pneumococcal peritonitis, since the enterococcus was more commonly found in the accompanying vulvo-vaginitis.

The CHAIRMAN (Dr. Thursfield) commented upon the curious geographical distribution of the disease, which was apparently common in Edinburgh, and commoner at Guy's Hospital than in North London. He had himself never seen a case which he could diagnose with certainty.

Mr. R. A. RAMSAY pointed out that the acute cases were almost invariably confused with appendicitis. The diagnosis could easily be made at operation by the absence of smell from the pus. In his experience the mortality of acute cases was very high, and operation did not help. His one recovery from the acute stage had occurred after putting Type I pneumococcal serum into the drainage tube, but he did not know whether recovery was attributable to this. A child first seen in the "residual abscess" stage had recovered after a number of metastatic abscesses in the limbs and elsewhere had been opened.

Dr. ERIC BELLINGHAM SMITH had seen eight cases, seven of them in girls, but one in a boy. The ages of the patients ranged between 3 months and 8 years. Diarrhoea occurred as a premonitory symptoms in all cases. He disagreed with Mr. Waugh as to the absence of pain, for in the cases which he had seen there was acute pain and tenderness throughout the illness. Only three of these cases had recovered. In his experience operation, while justifiable and necessary in the residual abscess stage, was totally unjustifiable in the acute stage. He quoted a case in which operation was performed on the fourth day, when there were signs that the toxaemia was diminishing, on account of persistent tenderness in the right iliac fossa; the child was dead within twenty-four hours. He considered that the only chance for the patient lay in expectant treatment in the Fowler position until such time as a residual abscess should have formed.

Mr. MAX PAGE did not think the mere drainage of the abdomen in the acute stage was in any way helpful, and would be inclined to leave the patient until the residual abscess stage provided he could be certain of the diagnosis. He had, however, had three cases who had survived operation in the acute stages.

Mr. E. C. HUGHES was convinced of the futility of operating in the early stage, and would never do so provided appendicitis could be definitely ruled out; many cases were, however, indistinguishable. In the differential diagnosis the general distension of the abdomen or the appearance of a swelling at the umbilicus were helpful signs.

Mr. J. McLEAN PINKERTON had seen five cases of this condition during two years as a resident at a small hospital

in South London. Three of the cases were proved to be pneumococcal peritonitis by pathological examination, and though the pus was not examined in the other cases there could hardly be any doubt as to the diagnosis. The clinical course and the appearances at operation of the cases coincided with that described by Mr. Waugh. Two had been operated upon in the acute stage and the others in the residual abscess stage, and all recovered, although one of the cases operated upon in the acute stage had required a further operation for a residual abscess in the pelvis.

Mr. J. A. C. FORSYTH had seen a few cases, all of which resembled those described by Mr. Waugh. In one case—a girl, aged 8 years, who had exhibited prodromal diarrhoea—he had operated at an early stage on account of slight tenderness in the right iliac fossa. At the operation the appendix and caecum were found to be normal, but the tubes were crimson. He drained the abdomen and left the tubes intact, although he was somewhat doubtful as to the wisdom of doing so. The girl recovered at the time, but had recently undergone an operation at the hands of another surgeon for double pyosalpinx. He considered that the absence of rigidity was one of the striking features of pneumococcal peritonitis.

Dr. ROBERT HUTCHISON commented upon the lack of unanimity amongst the speakers as regards both diagnosis and treatment; and Dr. CAMERON and Dr. NABARRO replied briefly.

TREATMENT OF PULPLESS TEETH.

AT a meeting of the Section of Odontology of the Royal Society of Medicine on January 26th, Mr. J. G. TURNER presiding, two papers were read on the subject of pulpless teeth.

The first was a communication by Mr. CALE MATTHEWS, who described a case of long-standing insomnia treated by the extraction of these teeth. The patient was a married woman aged 44. She had marked exophthalmos, but no goitre, and otherwise was in excellent health. In May, 1923, she had an intermittent temperature after pneumonia, and the doctor attending her thought that the temperature might be due to some other cause, and advised that her teeth be radiographed. It was then elicited that she had been suffering from a gradually progressing insomnia during the previous twenty years. Her average night's sleep was three or four hours, and if there was any worry or domestic disturbance she got no sleep at all. More than twenty years previously—before the insomnia began—she had certain root fillings and crowns. After an investigation of the radiographic result it was decided to remove all the pulpless teeth. The extraction proved extremely difficult, and for two or three subsequent days the patient was very much distressed, but four or five nights afterwards she had the best night's sleep she had had for years, and this improvement continued. Almost a year afterwards her healthy and rejuvenated appearance made her almost unrecognizable as the same woman; she was sleeping well, and, most notable of all, there was a total disappearance of the exophthalmos. At first the speaker was inclined to think that the improvement might be a temporary one, due to some psychic cause, but it persisted. He could offer no explanation except that the removal of the septic foci eliminated the cause of the hyperthyroidism which might have been the determining factor of the insomnia.

Mr. J. G. TURNER said that in his own experience he had found that insomnia was one of the things which might be cured by clearing out a mouthful of foul teeth, whether dead or subject to pyorrhoea. Only the previous week, by taking out one loose pyorrhoeic tooth, he had restored sleep to a patient who had been clamouring for drugs.

Mr. L. E. CLAREMONT read the second paper, on the general aspects of the problem of pulpless teeth. After a description of the anatomy and pathology of the parts concerned, he went on to speak of treatment, confining his remarks to the permanent dentition. The treatment of deciduous pulps was impracticable, except perhaps in children from 3½ to 5 years of age. The treatment of the permanent teeth could be considered under three headings: (1) devitalization of the living pulp; (2) cleaning and filling of sterile canals; (3) treatment of septic teeth. The rule, "When in doubt, remove the pulp," was, he considered, a good one. The development of infection during treatment was a very important thing to be guarded against, and a thing liable to be forgotten. Everything that came in contact with a tooth under treatment was infectious and a certain source of infection unless sterilized. In cleaning and filling the canals the watchword should be "sterility." It was not possible to be aseptic, but the next best thing could be done—namely, the work could be carried out in an antiseptic atmosphere. The choice of antiseptic was limited. Tricresol was the nearest approach to the ideal. The teeth should be isolated with rubber wiped over with tricresol. All instruments should lie in tricresol. A tooth should be allowed to drain, and the canal contents should afterwards be rendered sterile, before removal. Secondary dentine or pulp stones might be absorbed out by means of sulphuric or hydrochloric acid. The use of mummificatory paste should be restricted as much as possible; he viewed it with great distrust. Various methods had been devised for filling the canals. If gutta-percha points were used they should always be kept in formalin vapour and taken straight from this to the canal. The treatment of teeth already infected was the greatest problem before the dental surgeon of to-day. The first thing to do was to arrive at a correct diagnosis. Errors were frequently made through want of co-ordination of the factors and misinterpretation of the *x*-ray photographs. Mr. Claremont dealt first with treatment via the pulp canal, and secondly with surgical treatment. With regard to the pulp canal, the great question was whether it was possible to render the periapical area and the sinus, if present, sterile. Systemic toxaemia arising from apical sepsis had been conclusively proved. Before commencing treatment via the pulp canal one should consider the age of the patient and the question whether there was any systemic infection present. Ionization, using a solution of zinc chloride, with the positive pole so that chlorine was given off in the canal, was constantly used. His misgiving was whether all the organisms present in a spongy apex and beyond it had been killed. The apex was frequently necrotic, and it seemed almost impossible to prevent such a condition from being the source of further mischief. Sinuses might arise in two ways—from the bursting of an acute apical abscess and from a chronic abscess. Theoretically the teeth in which sinuses had developed would appear to offer more satisfactory scope for rendering the periapical tissue sterile. But whatever kind of treatment was carried out around the pulp canal left a feeling of dissatisfaction on the part of the operator. Surgical treatment might be considered as (1) preliminary sterilization and filling of the root canal, followed by removal of the diseased periapical tissue; (2) preliminary sterilization and filling of the root canal, followed by removal of the necrotic apex and the surrounding tissue (this, provided the patient was healthy, might be applied to the upper incisors, canines, premolars, and possibly the low incisors, with apical rarefied areas involving not more than one-fourth of the periodontal membrane); (3) extraction coupled with curettage (it was most important to curette thoroughly the apical region of the tooth sockets after extraction). Cases suitable for this last procedure were those involving the upper molars and most of the lower teeth, with large areas of destruction, and teeth with a sinus opening into the antrum, nasal fossa, or on to the skin.

In the course of general discussion, Mr. LIVINGSTON remarked that formalin vapour for the sterilization of instruments was practically useless, especially in a dry cabinet. He obtained perfect sterilization of gutta-percha by using absolute alcohol to which was added about 40 per cent. formalin. Mr. SPRAWSON said that it had always seemed to him difficult to know when the pulp canal was sterile, and he thought it would be rather useful to test by films and cultures a number of teeth after they had been carefully dressed. Mr. CALE MATTHEWS said that the technique of the treatment of pulpless teeth was very exacting to the general practitioner of dentistry. He wished the reader of the paper had said something about the ammoniated silver nitrate method of root filling. If the radiograph meant anything to them, it was a great satisfaction to study the radiographic appearance of teeth after they had been filled with the silver nitrate. Mr. SKINNER

thought it most important that the patient should be warned that once he had a dead tooth in his head, sooner or later trouble might arise.

Mr. A. T. PITTS asked whether there was any greater likelihood of apical rarefaction developing in teeth which had been devitalized and afterwards had become infected than in a tooth which had died what he might call a natural death and had not been treated at all. There was an impression amongst medical men that apical rarefaction was always the sequel to dental treatment, and dental technique was regarded as being on its trial. The physician saw various teeth with obvious fillings and crowns, and, noticing apical rarefaction, came to the conclusion that the condition was due to the dentist. He would like a series of radiographs to be taken on patients with entirely neglected mouths, when he believed it would be found that apical rarefaction was quite common.

Mr. J. G. TURNER closed the discussion by remarking that the question of the depth of infection of the dentine had been overlooked, and that until this had been dealt with one was still left, whatever might have been done, with an infective dead piece of tissue.

MENTAL SEQUELAE OF ENCEPHALITIS LETHARGICA.

THE discussion on "The mental sequelae of encephalitis lethargica" at the Royal Society of Medicine (see BRITISH MEDICAL JOURNAL, January 17th, p. 114) was resumed on January 27th. Dr. R. H. COLE, Vice-President of the Section of Psychiatry, was in the chair.

Dr. J. C. SPENCE (Newcastle) dealt with the subject as it concerns children, for 30 to 40 per cent. of the cases of the disease he had seen were under 12 years of age. He considered that the chief problem was not so much diagnosis, or even treatment—the latter being so ineffective—as prognosis. It was very difficult to foretell, in any given case, what the ultimate mental condition of the patient would be. Even in cases having a mild initial attack the later mental changes might be quite gross. One of the most important results of the disease, he found, was a diminution or perversion of volition; the patient might remain motionless for hours. He described the types of cases he had met with, and quoted illustrative examples of each.

Dr. C. WORSTER-DROUGHT stated that cases of mental disorder reaching certifiable limits were comparatively rare. When they did occur the condition was either progressive dementia, a delusional state, or hallucinatory confusion. He did not think it could be said that there were any really characteristic mental symptoms arising from encephalitis lethargica; perhaps the most frequent was an apparent apathy, but tests showed this was not really so pronounced as it appeared to be, and there was but little impairment of intellect. In most of the 19 cases tested by Dr. Hardcastle and himself the psycho-motor reaction time was definitely lengthened, usually by about 50 per cent., but the cerebration time was not appreciably altered. In the general intelligence tests only 3 of the 19 were below the normal, the deficiency in those 3 being chiefly in the regions of memory and reason. The prognosis in regard to moral deterioration following the disease was often quite good.

Dr. HARDCASTLE expressed his agreement with the remarks of the previous speaker. In some of the cases inquired into, he said, the onset of the symptoms was preceded by an extraordinary sense of well-being. Some cases seemed to recover completely, and then, perhaps six months later, mental sequelae became definitely established.

Dr. E. MAPOTHER mentioned the results of his investigations in 45 cases of the condition which had been received into the Maudsley Hospital. Those cases could be arranged under three headings: (1) mental anergia, (2) morbid restlessness, (3) demoralization. The occurrence of extreme lethargy seemed, generally, to be ominous as regards the subject's future. In many cases showing mental changes the initial illness was so slight that its nature had not been recognized. There seemed to be no relation between the organic and the mental changes. In the majority the disturbances were conative. There was a marked spontaneous variation, suggesting an active continuance of the disease. Many of the cases obviously needed institutional treatment, either because they were incapable of self-support or because they were a nuisance to others.

Dr. GRAHAM FORBES spoke of some cases in which the mental after-effects of encephalitis lethargica were complicated by epilepsy, and in some instances it had not been possible to determine whether the condition the patient was found in was post-epileptic or post-encephalitic.

Dr. J. GILMOUR referred to some adult cases in which, after apparent recovery, the patients developed mental conditions such as defective memory, restlessness, and dementia four or more years after the initial attack.

Dr. CRICHTON MILLER said that for the mental symptoms following the Parkinsonian type of the disease, sedative drugs, especially hyoscine, seemed to have a specific effect on the apathy and the anergia.

Dr. WORSTER-DROUGHT said that if once hyoscine was started in these cases it was difficult to leave it off, and for that reason he was very chary about giving it.

Dr. P. C. P. CLOAKE spoke of the relationship of psychic trauma to the onset of this disease. In one case it followed immediately upon the boy being pushed into a bath strongly against his will. He thought it likely that the disease was at least aggravated by such trauma. The findings of Dr. Worster-Drought and his colleague in regard to psycho-motor retardation did not agree with those of Narval and others. But there was a psychic retardation apart from the execution of motor acts. With regard to the period of abstention from school, he advised that the subjects of the disease should be kept from school considerably longer than would otherwise appear to be necessary. Finally, he considered that the essential feature of the psychic change was the result of a definite organic injury to the cerebral cortex, this injury being caused by the toxin of the disease.

The CHAIRMAN (Dr. Cole) said he considered the disease only mildly infective, but he related a few cases in which there seemed to have been conveyance from one to another; in one the nurse and matron concerned with a case both had the disease afterwards. These might be coincidences, and the cases might each have derived the virus from the same source, rather than from each other. Hyoscine was a drug which should not be used for any length of time; it should be looked upon only as a drug for emergencies. Patients who had recovered from mental illness and to whom this drug was given told him that they felt as if they were in a vice; they had the desire to move, but were unable to do so.

THE WASSERMANN REACTION AS AN INDICATOR IN THE TREATMENT OF SYPHILIS.

AT a meeting of the Liverpool Medical Institution on January 15th, Professor ERNEST GLYNN, Dr. R. E. ROBERTS, and Dr. PHOEBE BIGLAND, in a paper entitled "The failure of mercurial continuation treatment to keep a negative Wassermann negative in syphilitic out-patients," communicated the second half of their study of the Wassermann reaction in relation to the treatment at Dr. F. P. Wilson's clinic at the Royal Infirmary. A report of the first half of this study appeared in the JOURNAL of April 26th, 1924 (p. 748).

The authors studied the Wassermann relapse incidence in 503 patients whose reaction was negative after the standard admission course of six doses of salvarsan, etc., and eight of intramuscular grey oils. About two-thirds of these patients received continuation courses of either grey oil or mercury tablets—the latter usually prescribed to sailors or other "wanderers"—to keep their Wassermann reaction negative. About one-third received no treatment for a few or many months because of their irregular attendance. The authors were therefore able to compare the Wassermann relapse incidence in the three groups. The patients were divided into five periods dating from the beginning of their negative Wassermann—namely, 0 to 6 months, 503 patients; 7 to 12 months, 382 patients; 13 to 24 months, 264 patients; 25 to 36 months, 100 patients; 37 to 48 months, 27 patients. Among these there were 185 "first" Wassermann relapses; six cases of reinfection were excluded. The Wassermann relapses were classified into complete and partial, the technique being number 4 of the

Medical Research Council's report. The authors expected to find that patients receiving no treatment relapsed more frequently than the treated, but exactly the opposite occurred, as shown by the samples below.

Examples of Partial and Complete Wassermann Relapse Incidence in 503 Patients.

Period (Months).	No Treatment.		Grey Oil.		Mercury Tablet.	
	Partial.	Complete.	Partial.	Complete.	Partial.	Complete.
	Per cent.	Per cent.	Per cent.	Per cent.	Per cent.	Per cent.
0 to 6 and 7 to 12 combined	9.5	13.0	28.2	20.3	15.6	13.0
13 to 24	6.6	9.2	No cases		12.5	7.8

Of the 185 patients with the "first" Wassermann relapse, 109 again became negative on further treatment, usually with salvarsan and grey oil: they were classified in the same manner as the 503 patients. Thirty-five relapsed a second time. In general the incidence of the second relapses in the three groups was the same as that in the first relapses. In both series complete and partial relapses were more frequent after grey oil than after mercury tablets. These unexpected results were not due to the influence of selection by the clinicians, the Wassermann reaction on admission, the "delay period"—namely, the time elapsing between the original infection and the patient's arrival at the clinic, to occasional deviations from the standard continuation treatment, or to other variable factors. Fildes and Parnell, unknown to the authors, had independently come to a similar conclusion in 1919 on the failure of mercurial continuation treatment after salvarsan, by the study of the Wassermann relapse in 129 syphilitic patients at Haslar, while Sir Patrick Watson of Edinburgh, forty years earlier, had not approved the administration of mercury to prevent the recurrence of the manifestations of syphilis at a later date. The various factors which might account for these unexpected results were next considered. The authors thought that it was wrong to assume that because mercury was often beneficial in treating active syphilis in the presence of a positive Wassermann reaction, therefore it must also be beneficial in latent syphilis with a negative reaction. The pathological conditions were absolutely different. Evidence was produced that the increased Wassermann relapse incidence after mercury might be partly explained by the development of mercury-resistant spirochaetes, sometimes by a provocative reaction, but chiefly—as the statistics suggested—by a lowering of the patient's immunity. This last was more likely to occur if the mercurial continuation courses began, as usually happened, about six to eight weeks after the end of the admission course of salvarsan and grey oil, from which it sometimes took the liver six months to recover. Evidence was next produced that the Wassermann relapses were injurious, partly because the complete relapses were associated with clinical relapses in 6.7 per cent. of the "no treatment" cases, in 9.0 per cent. of the mercury tablet cases, and in 16.9 of the grey oil cases. In order to prevent the occurrence of Wassermann relapses, it was advised that two or possibly three continuation courses of salvarsan should be given during the two years, controlled if necessary by the van den Bergh, levulose tolerance, and other tests, the first being performed about six months after the admission course. The dormant, but inaccessible spirochaetes were more likely to be destroyed by the intensive administration of this powerful remedy than by the prolonged courses of mercury, a drug now recognized as being much less efficient in the treatment of active syphilis. This work had been assisted by the Medical Research Council.

HELIOTHERAPY.

AT a meeting of the Cardiff Medical Society on January 13th, the President, Dr. E. E. BRIERLEY, in the chair, Dr. IVOR J. DAVIES gave an address on heliotherapy as seen at Leysin, illustrated by lantern slides from the clinic of Dr. A. Rollier.

Dr. Davies first described the favourable situation of the resort in the Vaudois Alps, and the scenic beauty viewed from the balconies of the various sanatoriums and chalets was commented upon as being a factor of no little importance in the cure. The general principles of treatment were then described, and special emphasis was laid upon the care taken after acclimatization in the shade to ensure a gradual insolation up to exposure of the whole body in a period of fourteen days, the head and neck being protected by some suitable covering or awning. The early hours of the morning were the best for exposure, before the heat rays of the sun became intense. Pigmentation resulted according to the rate of progress, and was of great value in prognosis. When progress was favourable pigmentation occurred to a remarkable degree, being as a rule of a rich bronze tint and resembling the colour of the North American Indian or of the races of Southern India. Many fair-complexioned and blue-eyed children were seen with chocolate-brown bodies as a result of the treatment. On the other hand, freckling occurred when progress was slow or unfavourable; when this was replaced by the characteristic general pigmentation the prognosis was much better. A wonderful improvement, both generally and locally, was the rule in the cases seen personally by the speaker. The state of nutrition and of muscular tone and development was remarkable, despite the comparative immobility through the compulsory rest, and little or no apparatus was employed. Dr. Rollier prescribed a lacto-vegetarian and fruitarian diet, and allowed meat in small amounts only once a day. This simple diet appeared to be ample for the needs of the body in patients undergoing the sun cure. The children were all kept agreeably occupied with graded work, and all were noticeably bright and lively. The cases, mostly of bone and joint tuberculosis, were making excellent progress towards recovery, with good return of articular function. Periodical skiagrams showed recalcification in various stages until the normal contours of the bone were restored. The best results were seen in Pott's disease and hip-joint disease, cure taking place in the spinal affection on an average within eighteen months, and in some instances even when complicated by paraplegia with marked muscular atrophy. Dr. Davies remarked that Dr. Rollier believes in non-intervention in cases of cold abscesses as far as possible, and holds that the prognosis in mid-dorsal lesions of tuberculous disease of the spine is better than in lower lesions, because the abscess formation cannot be readily reached surgically. Aspiration was much preferred to incision, being far less liable to lead to secondary infection. Non-tuberculous cases, such as weakly children and convalescents from measles, whooping-cough, or other acute specific diseases, were also seen progressing favourably. Reference was made to the great work done in this country by Sir Henry Gauvain at Alton and Hayling Island, where the Alpine association of pure, bracing, cold air and sunlight was reproduced as far as possible by graduated sea-bathing on sunny days. The admirable work of the Welsh National Memorial Association for Tuberculosis at St. Bride's on the Glamorganshire coast was also mentioned. Smoke screened some of the Welsh industrial areas from sunlight, and deprived such places of ultra-violet rays. One of the towns was almost enveloped in smoke, and the hills around were bereft of vegetation.

A discussion followed, in which Dr. COLSTON WILLIAMS (Medical Officer of Health for the County of Glamorgan), Dr. RALPH PICKEN (Medical Officer of Health for the City of Cardiff), Professor A. W. SHEEN of the Welsh National School of Medicine, and Dr. A. BROWNLEE (Medical Superintendent of the Glan Ely Tuberculosis Hospital) participated.

The Action of Light.

Dr. GARFIELD EVANS discussed simply the physics of light. He described the different kinds of radiations, which differed physically only in wave-lengths and the frequency of vibration; he explained how radiant energy included visible light, the ultra-violet and infra-red radiations, electric waves of great range of wave-lengths, x rays, and gamma rays. He also paid tribute to the pioneers in this branch of science—Newton, Romer, Herschel, Ampère, and Schumann. Ordinary glass, he stated, was practically opaque to ultra-violet rays. He described the lower layers

of the atmosphere forming the "trophosphere," which were mixed by wind currents and contained clouds; above this was the "stratosphere," which was windless and cloudless, and at a constant temperature, calm and cool. With the exception of atomic energy the sun was the prime source of energy; every chemical reaction was started with radiant energy. He next dealt with the penetration of the different rays, and discussed the properties and uses of melanin, which formed the brown pigment of the skin; it was unknown what function pigment had when deposited in the deep cells of the body. He described interesting experiments dealing with sensitization to light; white mice injected with a small quantity of haematoporphyrin became so sensitive that they died from exposure to light. He stated that sunstroke was produced by local heating of the brain, and that the circulation of the blood was not sufficient to distribute the radiant heat locally received; heat-stroke was produced by general heating. Referring to rickets in relation to light, he described a seasonal tide in this disease, the tide going up from January to March and down in May and June; the percentage of inorganic phosphate in the blood rose and fell with the prevalence of rickets. Dr. Garfield Evans added that he proposed to present at a later date an account of the cases treated with artificial light in his department at the Infirmary.

ENCEPHALITIS LETHARGICA.

AT a meeting of the Bristol Medico-Chirurgical Society on January 14th, the President, Dr. J. O. SYMES, in the chair, Dr. F. H. EDGEWORTH opened a discussion on encephalitis lethargica.

Dr. Edgeworth said that, from figures supplied to him from Dr. D. S. Davies, the medical officer of health, it appeared that in the five years 1919-23 151 cases of lethargic encephalitis had been notified in the city and county of Bristol. Of these, 13 could not be traced; but of the remainder (138 cases) 68 had died and 70 had recovered more or less completely, a mortality of 49 per cent. Of the 70 cases recovering, 26 had recovered completely, whilst 44 presented residual phenomena. The figures for 1924 showed great changes; 158 cases had been reported, of which 27 had died. The disease had become more widespread, but with a far less mortality—17 per cent. The cause of this difference in the mortality might be the better medical recognition of the clinical features of the disease, but possibly it was due to a diminished virulence of the infecting organism. It had yet to be determined whether this diminished mortality was associated with a diminished incidence of residual phenomena. Dr. Edgeworth then gave a sketch of the chief phenomena of the disease. He pointed out that whereas it was known that the Parkinsonian syndrome might arise after an interval of health (citing a case where the interval was two and a half years), it was not known whether other residual phenomena might similarly arise.

Dr. D. S. DAVIES said that Bristol showed no difference from the rest of England as regards incidence and mortality. The disease was most frequent in April, and tended to occur in densely populated areas.

Dr. C. F. COOMBS mentioned a prodromal period of "neuritic" pains in some cases, and suggested that it might eventually be possible to administer preventive treatment. He mentioned one accident during lumbar puncture, as a warning not to remove fluid too rapidly.

Dr. H. H. CARLETON described the diplopia as one for distant objects only. The lethargy might recur for periods of months after apparent recovery. He also had noted the prodromal period of pain. As regards treatment, he had formed a favourable opinion of mercury, but did not think hyoscine of value.

Dr. R. A. ASKINS referred to the absence of evidence of contagion. Late mental impairment in the patients who survived seemed common.

Mr. E. R. CHAMBERS confirmed Dr. Carleton's observation as to the character of the diplopia. No one muscle was concerned; he thought it due to a paralysis of divergence.

Dr. JOHN WALLACE (Medical Officer of Health, Weston-super-Mare) remarked that figures of incidence for a district where many people were sent to recuperate from illness might be misleading; some developed the disease while visiting the town. Sudden vomiting might be the first symptom.

Colonel A. E. J. LISTER pointed out that cases in which optic neuritis occurred were not very rare.

Dr. R. G. GORDON said that a common type in children showed lethargy only from early morning till noon; from then till evening the patients were normal, and through the night very restless. He mentioned that cases had occurred among miners where, long after apparent recovery, there was a persistent lack of initiative. In children a frequent feature of the residual phenomena was loss of moral control. He had found hyoscine useful for sufferers from myoclonus, if given in large doses.

The PRESIDENT analysed the late results in 33 cases: 11 were dead, 2 were well, 10 at work but suffering from tremors and spasms. Such symptoms might appear even three years from "recovery"; 10 patients had definite psychoses of various degrees.

THE USE AND ABUSE OF DIGITALIS.

A MEETING of the Manchester Medical Society was held on January 7th, with Professor G. R. MURRAY, the President, in the chair. A paper on the use and abuse of digitalis was read by Dr. G. J. LANGLEY.

Dr. Langley, after a short historical reference, attributed opposition to digitalis to two reasons: its use in unsuitable conditions, and the use of an ineffective preparation. The occurrence of failure was perhaps sufficiently demonstrated by the very large variety of preparations on the market. The reputation of the digitalis group appeared to rest upon its power to increase the force and effectiveness of the cardiac stroke, but unequivocal evidence in the field of experimental pharmacology was extremely difficult to find. The great complexity of cardiac problems made the interpretation of experimental results very difficult. Four or five factors determining the force of a cardiac stroke at once came to mind—slowing, increased intake, raised peripheral pressure, myocardial nutrition, and carbon-dioxide exchange. When the results obtained in undoubted cases of auricular fibrillation were considered the whole aspect of the matter changed; it was in this group that brilliant results were almost always obtained. Yet here again it was pointed out that for complete success two factors were essential—(*a*) auricular fibrillation, and (*b*) adequate dosage. The dose of the drug necessary to produce effect, the various methods of physiological assay, and the toxic effects were then discussed. Reasons were given for the belief that the optimum therapeutic dose and the toxic dose lay very near together, that the most important action of the drug was the delay caused in bundle conduction, but that some effect was produced also on the ventricular muscle, as indicated by the inversion of the electro-cardiographic T wave. Eggleston's massive digitalis method was next described; the indications for its use were considered, especially in relation to the gain of time and avoidance of discomfort by the patient; this was regarded as a strong reason for the wider adoption of the method. The method was reserved for fibrillation at high speed with relative or absolute cardiac failure; the dose was calculated in terms of cat units, and particulars of sixteen recently treated cases were given. The response in all cases except one was most successful, and the rapidity of action saved much suffering. Eight hours after the dose the patient was relatively comfortable, and the pulse rate in nearly every case was 80 a minute or less. In one case the drug appeared to have a definitely toxic effect upon the heart, the pulse speed being raised and the form of the electro-cardiogram entirely changed. It was finally submitted that digitalis was specific for the control, though not for the cure, of auricular fibrillation; that it was likely to be useless and possibly dangerous in other diseases; that the therapeutic and the toxic dose lay very close together; and that massive digitalis dosage was both reasonable and desirable in suitable cases.

INTERNATIONAL HEALTH ACTIVITIES.

AT a meeting of the Medico-Legal Society on January 20th Mr. F. LLEWELLYN JONES (coroner for Flintshire) read a paper entitled "The law of nations and the health of nations—a survey of international health activities before and after the great war."

He said it was only in comparatively recent times that the State had concerned itself with matters of public health. Questions of sanitation had not received the attention of the Legislature to any appreciable extent up to the middle years of the last century, and it was only under the stimulus of men like Sir John Simon, the chief medical officer to the Privy Council, that a code of laws to regulate public health ultimately found its way on to the statute book. Consequently, it was scarcely to be wondered at that health was not regarded as a subject of international control. The law of nations did not embrace questions of international health, and it was not until 1823 that the first international scientific conference was held. The close of the recent war marked the beginning of a new era, and the developments which had taken place since the formation of the Covenant of the League of Nations provided for the creation of an organization for the international control of health. Mr. Llewellyn Jones referred to the first English Quarantine Act, of 1710, and to the cholera epidemic of 1849-50, which led to a scientific inquiry resulting in the adoption of a policy of the almost total abolition of quarantine, though he pointed out that the Quarantine Act was not repealed until the passing of the Public Health Act of 1896, which transferred from the Privy Council to the Local Government Board the power to deal with ships infected with plague or yellow fever arriving in British ports, the great Act of 1875 having already transferred such power to deal with cholera. The International Health Office, which was eventually transformed into the International Office of Public Hygiene, grew out of the deliberations of the International Health Conference of 1903, but it was not until the Covenant of the League of Nations was framed that there was any extensive provision for the exercise of international health functions by a body representative of the different nations of the earth. At the first meeting of the League in Paris in 1920 the International Health Organization was established, and he was of opinion that there was no department of the League's activities which could claim so much successful work. The great war had resulted in famine in Eastern Europe and an epidemic in Russia, and the League appointed an Epidemic Commission in 1921, which undertook the organization of quarantine stations, the equipment of hospitals, the supplying of food and other necessaries, and the providing of measures for disinfecting and cleansing. This was the first attempt to deal internationally with an epidemic on a large scale, and the work of the Commission met with a great measure of success. The Health Organization had also initiated the step of interchanging public health personnel, by which sanitary officers became acquainted with practical solutions of public health problems, as well as with the organization of public health services, in various countries. The service of epidemiological intelligence and public health statistics had been most useful, reports on the prevalence of notifiable diseases being transmitted regularly from practically all the countries of the world. The health staff at Geneva had undertaken the supply of official vital statistics and statistical procedure, and were collecting information as to legislation and procedure regarding registration of births and deaths, certification of causes of death, notification of diseases, and other matters. The work of this department was of considerable value and interest to those engaged in public health administration. Therefore it was clear that there was a growing recognition of the importance of co-operation between the nations on sanitary matters, especially since the great war. The League had stemmed the spread of cholera and typhus in Eastern Europe, and research was being encouraged. Indeed, it was gratifying to the friends of international peace to know that, although eminent scientists were still searching for instruments and agencies of destruction for use in future wars, there were in all lands men who were engaged in a campaign against the scourges and enemies of the human race.

In the discussion which followed the CHAIRMAN (Mr. Walter C. Williams) observed that the work of their Health Department was of especial value to those members of the legal profession who were engaged in carrying out parliamentary enactments for the prevention and cure of disease.

Reviews.

ACUTE POLIOMYELITIS.

THE history of the early epidemics of diseases which have been carefully recorded by competent observers are always fruitful objects of study. The volume *Infantile Paralysis in Vermont*,[1] dealing with the various epidemics in the State between the years 1894 and 1922, is dedicated to the memory of Dr. Charles S. Caverley, whose pioneer work in the investigation of the disease as it occurred there between 1894 and 1917 has amply earned this graceful recognition. Seven of his original papers and reports on the subject are here reprinted. It appears that the first epidemic of poliomyelitis of any extent in the United States was that in the State of Vermont in 1894. Dr. Caverley's account of this will well repay study, especially when it is remembered how great has been the increase in knowledge of the subject since that time; indeed, the reader will easily appreciate this when he reads that two well known neurologists expressed the opinion that the disease was cerebro-spinal meningitis.

Since then Vermont has earned an unenviable notoriety in the matter of the incidence of the disease, other epidemics having taken place in 1910, 1911, 1912, 1914, 1915, 1916, and 1917. The reports of these abound in details of epidemiological and clinical importance, which reflect the industry and the skill of the observers. That dealing with the severe outbreak of 1914 may be selected as especially full of interest and information. That the principal seasonal incidence of the disease should be in August, September, and October agrees with the majority of experiences elsewhere; but the statement made that in Vermont it seems to be a rural as opposed to an urban disease conflicts with much other experience, notably, indeed, that in regard to the city of New York.

It is interesting to read the regulations adopted by the Vermont State Board of Health in 1916 in regard to the attendance of children under 15 at public gatherings; would public opinion here consent to their exclusion from all fairs, cinemas, theatres, schools, and churches, even in the presence of an epidemic of poliomyelitis? In 1917 the local board of health of Barre, Vermont, went so far as actually to prohibit all public gatherings of any kind, and we are informed that the city of Barre enjoyed comparative immunity from the disease. Our own health authorities must surely look with envy on the drastic powers employed by the Vermont Board of Health.

The after-treatment of the residual paralysis was placed under the direction of Dr. R. W. Lovett, professor of orthopaedic surgery at Harvard Medical School, whose untimely death is much deplored. Through his organization a very large number of cases were kept under observation and treatment. A quantitative test of muscle power, especially of the power of different groups of muscles or movements, was devised by means of a spring balance; its design and use are illustrated by plates. There is nothing very new in the description of the methods of after-treatment employed, but emphasis is laid on the value of muscle training. The use of diathermy is not mentioned.

Included in the volume are chapters describing the experimental results obtained with nasal washings from cases of the disease, but since much of this work has been repeated and extended in more recent years this section loses much of its interest. The book concludes with two chapters on the treatment of acute poliomyelitis by means of the intra-

[1] *Infantile Paralysis in Vermont, 1894-1922.* Burlington, Vermont: State Department of Public Health. 1924.

venous injection of hypertonic saline solution and the intrathecal injection of serum from convalescents. The epidemiologist and the neurologist will find in it much to interest them.

LIFE INSURANCE EXAMINATION.

DR. F. W. FOXWORTHY has got together a band of forty-nine contributors to the volume, *Life Insurance Examination*,[2] he has edited. His claims that it will prove both a guide and a text book to the medical examiner seem to be well justified. It surveys briefly the history of life assurance, industrial insurance, group insurance, fraternal insurance, and friendly societies, and sketches the comparatively recent origin and development of medical examination for life assurance, which dates back only to the middle of last century. Interesting chapters are devoted to the relation that should exist between medical examiners and agents, whose interests may often tend to conflict, and also to the close co-operation that should be maintained between the medical examiner and the actuary. The organization and administration of the medical department in American life offices is fully described, and the functions and duties of medical directors at the head office, of the medical referees supervising various "territories," and of the medical examiners in individual towns or districts, are explained. The training of medical officers for life-office work appears to be advancing generally along specialized lines towards the development of the particular viewpoint necessary for the medical examiner. While diagnosis is important, prognosis is still more so, and must be specialized in order that it may be translated into such terms that the degree of extra risk can be assessed under special mortality tables. An appreciation of the incidence of mortality is also shown to be of great importance. In short, the medical examiner and the actuary must be closely associated, so that their co-operation in the assessment of risks, expressed in terms of monetary equivalents, may be soundly based on the statistics prepared by the joint labours of the clinician and statistician. Such statistics are abundant in this work, more particularly in connexion with gastric and duodenal ulcers, blood pressure, goitre, neoplasms, glycosuria, and post-operative and tropical risks. These statistics and the practical conclusions drawn from them are worthy of the careful attention of all British medical examiners and actuaries. The nature of post-operative risks is also examined, and the methods of treating them for life assurance are concisely summarized. The general plans for dealing with sub-standard lives are also described, and illustrated by explanatory charts.

The methods of meeting additional risks are fully explained—the extra premium method, the lien method, the rated-up method, and the special classes method (the latter consisting of three groups of extra mortality averaging 38 per cent., 63 per cent., and 88 per cent. above normal). It appears that the lien method is passing out of favour in America. Possibly it does not appeal to the psychology of the American, or it may be that the elasticity of the method is not sufficiently appreciated. It should be remembered that a suitably adjusted lien, constant or decreasing, will meet practically any type of extra mortality; it is perhaps not generally recognized that a constant, or even a slowly decreasing, lien will cover an increasing extra mortality. The "numerical" method of assessment is also explained; it appears to be used in America more particularly for assessing extra risk in connexion with such factors as height, weight, build, family history, and occupation, taken in relation to the class of policy. But the expounder of the plan refers to its comparatively recent origin, and does not seem too optimistic about its intrinsic value, although it is admitted that experience may justify its more general adoption. The volume also contains interesting chapters on health, accident, and disability insurance. There are, moreover, some illuminating remarks on health conservation and insurance welfare work; but although the aims are undoubtedly admirable, the schemes are of too recent development to permit their results to be gauged.

The chapters on blood pressure and urinary analysis are excellent, but such subjects as the classification and descriptions of malignant neoplasms are unnecessary in a work of this description.

The final chapter, on "Postponement in disease," is particularly interesting and exhaustive. The joint experience of the leading insurance medical specialists of America, tabulating their opinions as to acceptance, postponement, or declension in connexion with some 300 diseases, is worthy of examination by British medical officers and actuaries. The printing and the illustrations are excellent.

A FRENCH TEXTBOOK OF THERAPEUTICS.

WHEN a volume on clinical therapeutics has reached its eleventh edition, and has besides been widely read in translation, comment on its merits is surely superfluous. The new edition[3] of Dr. LYON'S treatise is a worthy successor to its forerunners, and will no doubt achieve as wide a popularity. In the preparation of a new edition the problem of judicious elimination to make room for new material is seldom easy, but Dr. Lyon has solved it. The whole edition has been entirely recast. The elimination has been effected at the expense of clinical descriptions which have now lost the attraction of novelty, of many passages devoted to certain etiological aspects of disease, and of descriptions of treatment now no longer recommended.

There are many noteworthy additions. There is a succinct presentation of the subject of the cardiac arrhythmias, and a very full description of their treatment by means of quinidine. The recent researches throwing new light on the functioning of the sympathetic nervous system have rendered necessary the inclusion of a special chapter under this heading. The chapter on hysteria has been revised, and the teachings of Babinski followed in the main. There is a special chapter on protein shock and protein shock therapy. The treatment of syphilis by bismuth and of gonococcal infections by vaccines is fully described. The insulin treatment of diabetes, including recent modifications in its mode of employment, is fully dealt with.

In his last (tenth) edition the author drew the attention of his readers to the modern orientation of therapeutics, which tended more and more towards the employment of vaccines, of serums, and of glandular extracts. He now declares this tendency to be even more marked, and refers to the employment of protein shock therapy and of autohaemotherapy as evidence of how increasingly the trend is towards therapy of a biological nature. The practitioner, of course, may not be in a position to carry out treatment which involves a complicated technique, but he must at any rate, Dr. Lyon says, be prepared to take samples of blood, of sputum, or of exudates for examination, or to perform lumbar puncture. Detailed instructions are given as to how such samples should be collected. He must also be prepared to carry out certain modes of treatment—the intravenous injection of arsenobenzol and of organic serums, to inject antidiphtherial serum, to perform transfusion, and to induce an artificial pneumothorax. Dr. Lyon's instructions under these headings are conspicuously practical and complete.

The present edition has suffered in no wise from the eliminations, and has gained notably by the additions above referred to. We do not doubt that it will receive as ready a welcome as its predecessors, and so retain its well merited popularity.

DE SCHWEINITZ'S "DISEASES OF THE EYE."

A BOOK which has reached its tenth edition is obviously a good one, and DE SCHWEINITZ'S *Diseases of the Eye*,[4] the first edition of which dates from 1892, hardly needs any words of ours to recommend it.

As an introduction to the study of ophthalmology, more especially for the senior student, it is unsurpassed, and it

[2] *Life Insurance Examination.* Edited by Frank W. Foxworthy, Ph.B., M.D. Indianapolis, Indiana. London: Henry Kimpton. 1924. (Med. 8vo, pp. 738; 156 figures. 45s. net.)

[3] *Traité Élémentaire de Clinique Thérapeutique.* Par le Dr. Gaston Lyon. Onzième édition. Paris: Masson et Cie. 1924. (Sup. roy. 8vo, pp. xiv + 1408. Paper cover, fr. 70; cloth, fr. 85.)

[4] *Diseases of the Eye.* By George E. de Schweinitz, M.D., LL.D.Univ. of Penna., Sc.D.Univ. of Mich. Tenth edition, reset. Philadelphia and London: W. B. Saunders Company. 1924. (Roy. 8vo, pp. 865; 7 plates, 434 figures. 50s. net.)

is worthy to rank with the well known translation of Fuchs's great work. The word "introduction" calls to mind the remark of the schoolboy who opined that Caesar's *Commentaries* were written as an introduction for those beginning the study of the Latin tongue.

Recent advances in ophthalmology have necessitated a good deal of revision of the text; the slit-lamp work is new since the appearance of the last edition in 1921; the new illustrations number twenty-five; some of the old pictures have been redrawn. The very complete index adds materially to the value of a book which can confidently be recommended as one of the best in our language on its subject. It is a costly book and will hardly be within the means of junior students; but the man beginning the study of eye diseases in order to specialize in that branch will find it a good and safe guide.

CHRONAXY.

DR. GEORGES BOURGUIGNON describes his treatise on the chronaxy of man[5] as a study of the general physiology, normal and pathological, of the neuro-muscular and sensory systems. It is a large work dealing with a subject the importance of which, both for electro-physiology and for electro-diagnosis, has recently been recognized. Although the term "chronaxy" dates from 1906, most of the experimental work on which this volume is based has been done since 1915 by the author, who is director of the electro-radiotherapeutic department at the Salpêtrière.

The essence of the investigation is the recognition of the time factor as entering into the conception of the excitation of muscles and nerves. As long ago as 1848 Du Bois Raymond, working with the frog's gastrocnemius and the constant current, announced that the excitation (stimulus) depends only upon variations in current density and not at all upon the time of the passage of the current. Hoorweg (1892-97), using condensers for stimulation, introduced the consideration of the "liminal voltage" or voltage necessary to elicit the liminal contraction, and, having regard also to the "capacity" and the quantity of electricity, he related these variables in an equation (law of Hoorweg). In 1901 Georges Weiss introduced the consideration of the time factor in a statement which related the liminal intensity and the time during which the current was passing. M. and Mme Lapicque completed these researches by their definitions of "rhéobase" and "chronaxie" (1906-23). By rheobase they mean the current intensity necessary to elicit the threshold (*seuil*) of contraction on closing the constant current, and by chronaxy they mean the time of the passage of the current (necessary to elicit the threshold contraction) when its intensity is *twice* that of the rheobase.

Dr. Bourguignon has investigated the problem involved in the experimental aspect of this comparatively new subject; he has dealt with bipolar *versus* unipolar methods, the distribution of the current between the poles on the skin, "virtual" poles, the resistance of the living tissues, and the reduction of potential from the source of stimulation.

Dr. Bourguignon has gone over the whole subject of the regional distribution of motor and sensory points. He has determined the chronaxy of every accessible muscle in the body, and has brought out some facts that could not have been foretold. Some of these are that all muscles engaged in a particular function—for example, flexion—have the same chronaxy, and that all their synergists have the same chronaxy, whereas all the antagonists (extensors) have a chronaxy just twice as great. These relationships extend even to the muscles of respiration, for the muscles of expiration have a chronaxy double that of those of inspiration. In a limb the distal muscles involved in any action have a greater chronaxy than the more central. If σ stands for 0.001″, the actual value of the chronaxy of the flexors of the forearm is from 0.08 σ to 0.16 σ, and for the extensors from 0.2 σ to 0.36 σ. The chronaxies of the sensory nerves have been ascertained by stimulating the nerves through the skin and noting the moment of perception of the peripherally referred sensations of formication (method of Duchenne). In any given nerve or posterior root, fibres with different chronaxies are intermingled. The map of the distribution of sensory fibres possessing the same chronaxy is regional, and in any given region the chronaxy of the nerves is the same as that of the subjacent muscles.

Dr. Bourguignon has even attempted to estimate the chronaxy of the optic nerve by observing the production of the phosphene. It varies from 1.24 σ to 2.36 σ.

A large part of the book is occupied with the determinations of the chronaxies of muscles and nerves in various pathological conditions. While the chronaxy is normal in such states as athetosis, myoclonus, choreiform movements and tremor, it is greater than normal in states of contracture. An interesting fact has been brought to light in regard to the chronaxy of synergists in certain abnormal states—namely, that it is not of the same value as that of the muscles in whose function the synergists co-operate.

The comparative physiology of the chronaxy of muscle has also been studied, and has been determined in the frog, toad, snail, heart of tortoise, and stomach of frog.

There is no index, but in its place a detailed and well arranged table of contents.

A GERMAN TEXTBOOK OF OPERATIVE SURGERY.

A TEXTBOOK in English will almost certainly always be preferred by the English student to any foreign work. The preference is wise, for it is desirable that the student should be careful to base his knowledge on the doctrines and methods of the practical workers and teachers of his own country. There are occasions when the rule may be relaxed in favour of certain textbooks written and published in the United States, and such a work as Osler's *Medicine* makes an equal appeal on both sides of the Atlantic. Many of the smaller French textbooks are excellent, but they are best read by the senior student who has knowledge enough to appreciate the differences in methods and opinions and wishes to widen his horizon. Not so much can be said for the majority of German textbooks. There are exceptions, but usually their authors seem to have failed to make themselves acquainted with things done beyond the German frontier, or written in any language other than German. Consequently their textbooks cannot be recommended either from the educational or examination point of view to English students, even those who read German fluently.

If an exception is to be made it must be because the book possesses some special qualities. Such an exception seems justified in the case of the late Professor ZUCKERKANDL'S book on operative surgery, the sixth edition[6] of which has recently been prepared by Professor SEIFERT. The book is intrinsically good, and surgery is so much of an art that it is useful for the senior student and young surgeon to learn how things are done in other countries.

Professor Seifert asserts that it is the oldest of the textbooks on the subject now in use, and, while making the additions necessary to bring the book up to date, he has preserved the essential characters of the original work as far as possible. In particular, he has been careful not to go beyond its original scope—namely, the description of typical operations and operative procedures. Although the book is elementary in the sense that the simplest matters are explained, it is nevertheless comprehensive, extending from such simple procedures as the puncture of a hydrocele to resection of the pylorus, choledochoduodenostomy, and extirpation of the larynx. The subject is divided into general and special parts—the former dealing with the position of the patient during operation, the incision and division of tissues, the arrest of haemorrhage, and the management of the operation wound; the latter dealing with the operations themselves, in the usual regional order of classification. Ophthalmic and gynaecological operations, however, are not included. The special qualities of the book are to be found in the diction and the illustrations. The descriptions are exceptionally clear and

[5] *La chronaxie chez l'homme: étude de physiologie générale (normale et pathologique) des systèmes neuro-musculaires et sensitifs.* Par le Dr. Georges Bourguignon. Paris: Masson et Cie. 1923. (Roy. 8vo, pp. iv + 417; 52 figures, 192 tables. Fr. 35 net.)

[6] *Chirurgische Operationslehre.* By Professor Dr. Otto Zuckerkandl. New sixth edition, revised by Professor Ernst Seifert. Munich: J. F. Lehmann. 1924. (Roy. 8vo, pp. viii + 412; 487 figures. 15s. 2d.)

distinct, and while no essential detail is omitted the whole subject is treated with great conciseness. The book contains nearly five hundred illustrations—pains have been taken to ensure their being excellent in quality and adequate as regards number and choice—as a necessary complement to the text. It will not be surprising if the book in its new edition retains its popularity.

WILLIAM HARVEY.

DR. R. B. HERVEY WYATT has written a popular account of *William Harvey*[7] for the Roadmaker Series of biographies. The story of Harvey's life is set out simply and clearly, showing how his discoveries were related to his clinical practice, and how he destroyed physiological fancies by revealing facts. The greater part of the volume is rightly devoted to his great treatise *De Motu Cordis et Sanguinis*. The account of the discoveries made by Harvey is preceded by a summary of the work of Galen, Servetus, Caesalpinus, and others in this connexion. The author, writing for the laity, has been obliged to omit much that would have been of medical interest, but the fine instructions left to the College of Physicians with his gift of the Burmarsh estate might well have been quoted: "To search and study out the secrets of Nature by way of experiment; and also for the honour of the profession to continue in mutual love and affection among themselves." This is a message the tones of which have not yet ceased to reverberate.

Dr. Wyatt is to be congratulated on the clear way in which he shows that Harvey has an even greater claim to honour in this recognition of the value of experimental research whereby he has enriched not only medicine but all the sciences. For those who will be stimulated to read more deeply about Harvey an excellent list of books is given, and a useful chronological table of events during Harvey's life is appended. A valuable service has been rendered by this production of the story of the life of a great man and a great scientist.

NOTES ON BOOKS.

The Elements of Colloidal Chemistry,[8] by HERBERT FREUNDLICH, is a non-mathematical introduction to the subject by the author of a work on colloidal chemistry which has deservedly attained recognition as a standard treatise. His deductions are based for the most part on the properties of the surfaces and interfacial boundaries of particles in the colloidal state. Phenomena dependent on the action connoted under the term "adsorption" are thus brought into correlation with colloidal effects. The theorems are illustrated by reference to many familiar phenomena; these are so diverse as to include a London fog and the agglutination of typhoid bacilli. The book is a very helpful short treatise.

A little book of 60 pages has been written by Miss MCHENRY and Miss COOPER of Toronto, entitled *Diabetic Diet: A Handbook for Diabetics*.[9] It is intended for patients so that they may be able to co-operate with the physician in the control of their disease and also secure the most varied diet permissible. It has chapters on the composition and value of food, on analysis of urine, on a general plan of diabetic diets with sample menus, and a collection of receipts. It contains also a table of food values illustrating the approximate carbohydrate content of vegetables and fruit. The authors have enjoyed the experience of working under the close supervision of Dr. Banting, for we learn from the introduction that as early as the spring of 1922 Dr. Banting arranged for the establishment of a diabetic clinic at Christie Street Hospital, Toronto. For the last two years this clinic has been managed by Dr. Gilchrist, a friend and colleague of Dr. Banting; in this institution the authors gained their experience, and both Dr. BANTING and Dr. GILCHRIST have written introductions for the book. The practical advice and instruction contained in it ought to be very helpful to patients suffering from diabetes, and should enable them to make the best of the limited number of articles of food to which they are condemned. The menu offered in the chapter on receipts allows of a good deal of variety, and if all are as palatable as they sound their judicious selection should be invaluable in banishing monotony. Although not written for physicians many may find it useful; it may enable them to win a reputation for an acquaintance with culinary operations which their patients had not suspected.

The forty-first annual issue of the *Year-Book of the Scientific and Learned Societies of Great Britain and Ireland*[10] is described in the preface as containing records which picture "a pageant of the learned through twelve months of British history." Accounts are given of the progress made in science, literature, and art by a large number of societies and Government institutions. The present volume is again slightly larger than the previous issue, and seven new societies have been added to the list. Under each society is given a list of officers, places, and dates of meetings, membership conditions, and papers read or publications issued during the year under review. It is therefore possible to ascertain the nature and value of the transactions of such societies as the Royal Society of Medicine, the Medical Research Council, and the Lister Institute. The British Medical Association section, occupying nearly four pages, includes a list of the papers communicated to the Sections at the Annual Meeting at Bradford. With the rapid increase of scientific specialization this *Year-Book* becomes a necessary part of the equipment of all scientific workers.

The fifty-second annual issue of *Willing's Press Guide*[11] follows the usual plan. Besides an alphabetical list of newspapers and periodicals issued in the United Kingdom, there is an index of various interests, professions, trades, religious denominations, sciences, and subjects, with their respective newspapers and periodicals. An account of changes of title and of amalgamations relating to publications which have not completely passed out of existence affords valuable help in tracing the history of some of the oldest newspapers and periodicals, a chronological list of which is provided. Metropolitan and provincial newspapers are enumerated, and also Colonial and Continental newspapers in English. The foreign section consists of a list of the principal newspapers of foreign countries, any London addresses being given. A section is devoted to news agencies. The volume remains a most handy work of reference, and includes a variety of general information with regard to journalism.

[7] *William Harvey* (*1578-1657*). By R. B. Hervey Wyatt. The Roadmaker Series. London: L. Parsons; Boston, U.S.A.: Small, Maynard and Co. 1924. (Cr. 8vo, pp. 214; 3 illustrations. 4s. 6d. net.)

[8] *The Elements of Colloidal Chemistry*. By Herbert Freundlich. Translated by George Barger, F.R.S. London: Methuen and Co., Ltd. 1924. (Cr. 8vo, pp. vii + 210; 27 figures, 1 plate. 7s. 6d. net.)

[9] *Diabetic Diet: A Handbook for Diabetics*. By A. Doris McHenry, B.A., and Marjorie M. Cooper, B.A. With an introduction by J. A. Gilchrist, B.A., M.B., and F. G. Banting, M.C., M.B., M.R.C.S., F.R.C.P., M.D., D.Sc., LL.D. Toronto: The Musson Book Company, Ltd.; London: J. Bale, Sons and Danielsson, Ltd. 1924. (Cr. 8vo, pp. 62. 9s. net.)

[10] *The Year-Book of the Scientific and Learned Societies of Great Britain and Ireland*. Compiled from official sources. Forty-first annual issue. London: C. Griffin and Co., Ltd. 1924. (Extra post 8vo, pp. vii + 405. 15s.)

[11] *Willing's Press Guide, 1925*. Fifty-second year. London: James Willing, Ltd. 1925. (Demy 8vo, pp. xi + 459. 2s. 6d.)

VITAL STATISTICS FOR ENGLAND AND WALES, 1924.

WE are indebted to the Registrar-General for the following statement regarding the birth rates and death rates and the rates of infantile mortality in England and Wales and certain parts of the country during 1924. The statement is issued for the information of medical officers of health. The birth rate and infantile mortality rate for London have been provisionally corrected for transfers.

ENGLAND AND WALES.

Birth Rate, Death Rate, and Infantile Mortality during the Year 1924 (Provisional Figures).

	Birth Rate per 1,000 Total Population.	Death Rate per 1,000 Population (Crude Rate).	Deaths under One Year per 1,000 Births.
England and Wales	18.8	12.2	75
105 county boroughs and great towns, including London	19.3	12.3	80
157 smaller towns (populations from 20,000 to 50,000 in 1921)	18.9	11.2	71
London	18.6	12.0	69

The death rate for England and Wales relates to the whole population, but that for London and the groups of towns to the civil population only.

The birth rate of England and Wales as a whole is the lowest recorded except during the war years 1917–19.

Nova et Vetera.

THE SERJEANT-SURGEONS TO THE KING.

THE death of Sir Frederick Treves in December, 1923, left Sir R. Havelock Charles the sole Serjeant-Surgeon to the King.

It is not known when this ancient and honourable office was first instituted, but it is probable that it existed long before the year 1461, when William Hobbes, Warden of the Barber Surgeons' Company, was appointed by Edward IV. As it was the duty of the serjeant-surgeon to accompany the sovereign on the battlefield, it is likely that Hobbes saw some service in those troublous times, but we know nothing of his career.

Nowadays the sovereign is well provided with medical attendants. There are in England seven physicians, five surgeons, four surgeon-apothecaries, three surgeon-oculists, a laryngologist, and two dentists, who form the personnel of the medical department of the Royal Household. In Scotland there is a physician, two surgeons, an oculist, two apothecaries, and a surgeon-dentist. In Ireland there are two physicians, two surgeons, and an oculist. But some of these offices are purely honorary.

Sergeanty or sergeancy is a form of tenure, and has been thus defined by an old writer: "Grand sergeancy is where a man holdeth his lands and tenements by doing some speciall service to the king in person." Thus the word "serjeant," as applied to lawyers and surgeons, implies personal service, and no doubt it meant in this case that the surgeon was a personal attendant on the king, but, despite any implications which some may think this to involve, the holder of this title ranked above the Regis Chirurgus Principalis, or chief surgeon, and to-day he comes directly after the physicians and before the surgeons in ordinary and other members of the medical department.

It must not be supposed that in Tudor times, because the serjeant-surgeon belonged to the Company of Barber Surgeons, he shaved the monarch's chin or cut his hair. These duties were performed by the king's barber, who also belonged to the Company. In the later centuries of the existence of a fraternity of barbers its members had differentiated into two classes—those who practised what was called "barbery," including blood-letting, and those who practised only surgery, from which latter class the royal surgeons were chosen. Barbers only added surgery to their functions when the clergy were forbidden to practise it. As Mr. Austin Young says:

> "Up till about the twelfth century the practice of surgery and medicine was . . . almost wholly confined to the clergy. . . . In 1163 the Council of Tours, under Pope Alexander III, considering that a practice which involved in its operations the shedding of blood was incompatible with the holy office of the clergy, forbad them to interfere in any matter of surgery; the consequence of this edict was that they gave over the operations of surgery, but continued to practise the healing art of medicine."

In London, however, there was a separate Guild of Surgeons, which was incorporated with the Barbers' Company by Act of Parliament in 1540.

At first there seems to have been only one serjeant-surgeon at a time, but from 1560 to 1891 there were two, and occasionally a third or extraordinary one was appointed who received no pay. After the death of Sir Prescott Hewett in 1891 the office was again single for a few years until the late Sir Frederick Treves was appointed in addition to Lord Lister, who had succeeded Sir James Paget. Treves was at first honorary serjeant-surgeon, and Sir Thomas Smith was made honorary serjeant-surgeon in 1901.

In Mr. Austin Young's sumptuous work *The Annals of the Barber-Surgeons of London* there is a list of such members of the Company as have served the office of serjeant-surgeon, and since practically all the serjeant-surgeons before 1745 belonged to the Company this list, which carries us down to 1736, is fairly comprehensive. A more complete list was published by Mr. (now Sir) D'Arcy Power in this JOURNAL (vol. i, 1900, p. 583). Some of the entries are to us but shadows of names, but others are records of men distinguished in their art, such as Thomas Vicary, of whom, however, a detractor wrote that he was "at first a meane practiser in Maidstone untill the King advanced him for curing his sore legge." It is to him that Henry VIII is handing (without looking at him) the charter of the Company in the great picture attributed to Holbein now hanging in Barbers' Hall. Like so many other well known surgeons of the sixteenth and seventeenth centuries, Vicary was on the staff of St. Bartholomew's, which was the only hospital—in the modern sense of the word—in the city of London. He continued to be serjeant-surgeon to Edward VI, Mary, and Elizabeth, and was surgeon to King Philip. Of Ferris we know little, save that Henry VIII left him 100 marks in his will, and that his own will is extant with a bequest to his daughter Thomazine of his "Courte bedd and bedstede and half a garnish of pewter vessels." His successor, Robert Balthrop, is only remarkable for his epitaph in the Church of St. Bartholomew the Less.

> "Here Robert Balthrop lies intombed, to Elizabeth our Queene
> Who Sergeant of the Surgeons sworne neare thirtye yeeres hath beene.
> He died at sixty-nine of yeeres December's ninthe the daye
> The yeare of Grace eight hundred twice deductynge nine a waye
> Let heere his rotten bones repose till angell's trompet sounde
> To warne the moulde of present chaunge and raise the deade from grounde."

The quaint but cumbrous conceit in which the date of 1591 is recorded, and the erroneous description "Sergeant of the Surgeons," are perhaps due to the difficulties encountered by the writer in versification. Balthrop's Will also is interesting in that it contains a list of books and surgical instruments, including a salvatory. He left "one of my Launcetts that is sett in golde and enamyled, to my felowe Goodorus." This was William Gooderus, who was Master of the Company in 1594 and serjeant-surgeon.

William Clowes the younger was serjeant-surgeon to Charles I. His better known father, whose books "are the best surgical writings of the Elizabethan age" according to the late Sir Norman Moore, was surgeon to Queen Elizabeth, but not serjeant-surgeon. The family to which these surgeons belonged was a gentle one "bearing tokens and arms of honour, helm, mantle, and crest," and Clowes was one of the first men of family to take up surgery as a profession.

Of Wiseman, who accompanied Charles II in many of his wanderings before the Restoration, Sir D'Arcy Power says that he was the first to rescue surgery in London from its subordination to the Royal College of Physicians. His works show him to have been a clinical observer of great acumen and a skilful practitioner. "He believed in the miracles wrought by the blood of Charles I, yet he married the granddaughter of a regicide."

King George II was the last of our kings who went into actual battle. His serjeant-surgeon, John Ranby, as in duty bound, accompanied him at Dettingen. The tradition of personal service in the field seems to have been broken through in the last war, for when his present Majesty crossed over to France he was not, we believe, accompanied by either of his serjeant-surgeons. The last Master of the Barber Surgeons Company who held the office of serjeant-surgeon was Sir Caesar Hawkins, who was Master in 1736 and was appointed serjeant-surgeon in 1747, two years after the separation of the surgeons from the barbers. He was the first surgeon to be made a baronet. His son, his brother, his nephew, and his grandson were all serjeant-surgeons. As his brother Pennell Hawkins was appointed before 1783, the two brothers must have held office together. Five members of this family held this office, the last of them, Sir Caesar Henry Hawkins, dying in 1884. When the first Sir Caesar's brother Pennell died in 1793, King George III appointed David Dundas to the vacant place. Dundas was an apothecary (or general practitioner) at Richmond, and as such (by by-law) ineligible to the high offices in the Company of Surgeons, to which the serjeant-surgeons had a traditional right. He protested against the decision to keep him out, and a legal contest seemed imminent, but he did not press his claim, and contented himself with the royal favour and a knighthood.

The great Sir Astley Cooper became serjeant-surgeon in 1828. Since his time Brodie, Lawrence, Fergusson, Paget, Hewett, Lister, and Treves have adorned the office and rendered it one of the most honourable open to the medical profession. The present occupant of the office is the only member of the Indian Medical Service who has held it.

British Medical Journal.

SATURDAY, JANUARY 31ST, 1925.

THE MEDICAL RESEARCH COUNCIL.

THE Medical Research Council, as we recently had occasion to recall, owed its origin to the "research penny," which, in accordance with a provision of the Insurance Act, Parliament undertook annually to provide. After five years' experience of its working the enlarged scope already given to it was confirmed by a charter of incorporation, and it was given its present position under the direction of a Ministerial Committee of the Privy Council, to which the Medical Research Council makes an annual report. It was the tenth of these reports which was noticed in two recent issues.[1] In its report for 1919 the work done in the years 1914-19 was reviewed; in the new report is a review of the following five years 1920-24.

Development was very greatly affected by the war, which broke out shortly after the work was begun. The effect was both temporary and permanent. At the urgent request of the War Office, and with the whole-hearted approval of the nation, attention was at once turned to war problems. Some of them were peculiar to war conditions, but the majority were not, though their solution was made more urgent by war. As an example, the nature and treatment of shock may be taken. Shock was being produced with terrible frequency among the wounded, but it is a condition with which surgeons in peace have commonly to contend, and by which their best efforts in the treatment of patients after accidents and after operations are often defeated. It is significant that a most important contribution to this problem, probably the most important, was made by the late Sir William Bayliss, whose life had been devoted, not to surgery or medicine, but to general physiology. The success which attended the application of the principles and methods of scientific medicine to the medical war problems with which the public mind was then full had a very great, and as many were fain to believe a lasting, effect. One reason for being thus sanguine was that the public appreciation of the absolute necessity, if the war was to be won, of applying scientific methods to the production of engines of war and to counteracting the mechanical and technical devices of the enemy, was not altogether a new thing. An appreciation of the importance of science to industry had been growing steadily, though very slowly, in Great Britain before the war. The extraordinary development in Germany of certain industries immediately dependent on scientific research, and in particular that of dyestuffs, had greatly disturbed the complacency of British manufacturers. The fact that the demand for explosives was a call on that industry was, in a sense, fortunate. The public mind had also come to believe that it was already receiving great benefits from the application of scientific research to the prevention of disease and the preservation of health; it heard much of the extension of the principles of bacteriology and chemistry to these matters during the war, and the successful results came very nearly home to those who had relatives in the armies—and who had not? But though there was therefore reason to hope that the lessons taught would not be forgotten when the war organization for medical research was dissolved, there could be no certainty.

[1] January 10th, p. 87; January 17th, p. 123.

In the quinquennial survey published in 1919 it was said that on the whole most of the men best fitted for original inquiry were given the opportunity for it and were given pay from public funds. When the war organization was broken up many men marked by distinguished scientific achievement in the war were in danger of returning either to non-scientific work or to teaching posts so poorly paid that it would have been necessary for them to augment their stipends by doing professional work of a kind incompatible with serious further effort in research. If this was to be avoided it could only be by public subvention or private munificence; fortunately both proved available. The House of Commons, encouraged by the growth of public opinion, made further provision for the universities (now administered through the University Grants Committee); it increased the annual vote for the Medical Research Council, and made other votes for scientific, industrial, and agricultural research. The action of Parliament and the growth of opinion to which we have referred had two other effects: it stimulated the pious benefactor to give or leave money to supply the needs of scientific research, and notably those of medical research, and it made the lay authorities of many hospitals "newly alive to their responsibilities for aiding the advancement of knowledge, by which alone the work of mercy can be made effective." By fresh efforts they have acquired better support for the research departments, and money has been obtained from particular benefactors and from the general public by wide appeals for aid to investigations into particular forms of disease.

Differences of opinion have existed as to the relative values of attending to "specific medical problems" and to "supporting the advance of the general level of the sciences contributory to medicine." There is room for both, and the decision made in any particular case must depend upon its attendant circumstances. "In practice, however, it is found that to serve one of these objects is almost invariably to serve the other." There is a stage in the affairs of any science which, taken at the flood, leads to practical application. That stage had, by a happy coincidence, been reached in physiology when the war broke out. "The pressing need for bringing the best physiological knowledge to the aid of sailors, soldiers, and workers in their exposure to every kind of violence, hardship, and physical stress, brought the physiologists increasingly to their proper place within the field of preventive and curative medicine. . . . Fruit in practical usefulness is now being gathered from the long years of work which physicians were apt in the past to label as academic." Instances will occur to everyone; those mentioned in the report are the growing knowledge of hormones, which has given us adrenaline, pituitrin, and insulin; the studies of muscle function, almost notorious for its supposed uselessness to the practitioner, which have provided basic knowledge that now underlies many parts of the science and art of medicine and is removing empiricism from practical studies of the heart, of some nervous disorders, of physical training, and of industrial labour.

Faith must be put unreservedly in the scientific workers themselves; the best and earliest success can only come from their free and disinterested work in the pursuit of clues that may lie here and there. "Recent history shows that sterility is soon reached when scientific work is tied to particular practical issues and ceases to be a free search for knowledge as such." The university clinics set up on the unit system with the aid of the University Grants Committee are serving as the bridge, and the student of

to-day is learning a new spirit as he accompanies his teachers backwards and forwards across it. Most of the work which has made the real difference to the daily tasks of preventive and curative medicine in the last few years has come from the laboratories of physiology and biochemistry rather than from those of bacteriology and pathology, and, the report continues, "it is notable that the chief contributions coming from the new clinics have, as it happens, been in clinical studies of the functions of the heart, the regulation of the respiratory mechanisms of the body, and the excretory efficiency of the body, carried out by men trained to use the precise methods of physiology or biochemistry." If we are in any risk of exaggerating the importance of the laboratory we have the example of the great man whose death we have this week so deeply to deplore. The story of his life has proved that the same man may be both physiologist and clinical physician, bringing to his clinical problems all the refinements of the most recent physiological methods, and making them so plain, and so greatly simplifying their application, that the practitioner may be content to use the end-results at the bedside without himself going through the physiological mill Mackenzie so courageously entered.

The Medical Research Council has from the first acted in co-operation with the universities and the medical schools attached to the universities. Of the total resources of the Council at least £80,000 is expended annually at the universities in grants and other forms of assistance to selected workers. Such a worker may also receive pay from the university in respect of his teaching duties, but whether he does so or not the intellectual life and efficiency of the universities gain from the improved opportunities made for members of their staffs or from the addition of new workers in the research field. This policy has been consistently followed. The example of the Rockefeller Institute of New York in establishing a research hospital has not been imitated, because it was felt to have the grave disadvantages of extravagant cost and of wasteful divorce from the current system of medical education; instead, the policy of assisting research work in the medical schools has been pursued. But there are also more general problems in physiology, pharmacology, biochemistry, and other departments, and for their investigation the National Institute for Medical Research, with field laboratories associated with it, has been established and maintained. To this purpose £48,500 was applied last year. The remarkable success of the policy followed from the first is plain to everyone, and is, we will venture to add, in large measure due to the foresight and good judgement shown by Sir Walter Fletcher, who left his physiological work in Cambridge to accept the arduous office of secretary of the Medical Research Committee, which, when it was appropriately placed under the constitutional control of a committee of the Privy Council, was given the name of the Medical Research Council.

THE OPIUM CONFERENCES AT GENEVA.

THE opium conferences at Geneva have resulted in what has been described as a deadlock. The first conference consists of representatives of those countries which have opium-producing territories; the second is to consider the drug evil generally. Full details of the session of January 24th are not yet available, but it is reported that all the contentious questions which have perplexed the two conferences for the past two months have been shelved by reference to a joint committee of sixteen members. Further plenary sittings of the conferences are postponed while this joint committee and the other committees proceed with their deliberations. It is added that the question whether the second conference is competent to deal with the American proposals of restriction of output to medical and scientific purposes and the suppression of opium smoking will not be discussed by the joint committee. These proposals, which seem to have been the occasion for the deadlock, are, according to Lord Cecil, to be raised later if necessary.

It had been hoped that the reinforcement of the British delegation by a Cabinet Minister might lead to some accommodation between the American proposals and those of Great Britain and India; this was desirable from every point of view. But, owing perhaps to insufficient information beforehand, Lord Cecil confessed himself to be involved in problems of greater difficulty than he had ever previously encountered at any international meeting, and he was betrayed into quoting figures as to the relative consumption of opium in the United States and in India which, at a subsequent meeting, he found it necessary to withdraw. We have previously[1] called attention to the uncertainty as to the competence of these conferences to amend the Hague Convention of 1912, to the difficulty of separating the question of opium smoking from other modes of abuse of these drugs, and to the need for more authoritative and skilled representation on behalf of Great Britain. It would indeed be lamentable if the agreement achieved at the Hague in 1912, 1913, and 1914, and the co-operation of nearly fifty Powers, already secured by the Dutch Government and the League of Nations, were now to be disturbed or further progress impeded by any sharp differences between the United States and this country. We sincerely trust that the joint committee of sixteen may find a way out of the present impasse. An initial mistake was probably made by the Assembly of the League in attempting to separate and refer to different conferences the question of opium smoking, its restriction or suppression, and that of the limitation of opium and coca products to medicinal and scientific purposes.

The whole problem has been greatly complicated by the enormous increase of opium cultivation in China after its practical extinction in 1917. The excess of the world's production of the drug has thereby been augmented, and smuggling in the Far East appears to be rampant. Unfounded assertions have been made to the effect that opium grown in India is responsible for drug addiction in America, but no one acquainted with the facts would need to be reassured by the emphatic repudiation of this assertion by Lord Olivier, the late Secretary of State for India, which appeared in the *Times* of January 16th.

It will be remembered that by Articles 6 and 7 of the International Opium Convention of 1912 it was enacted that "The Contracting Powers shall take measures for the gradual and effective suppression of the manufacture of, internal trade in, and use of prepared (*i.e.* smoking) opium, with due regard to the varying circumstances of each country concerned, unless regulations on the subject are already in existence"; and "The Contracting Powers shall prohibit the import and export of prepared opium; those Powers, however, which are not yet ready to prohibit immediately the export of prepared opium shall prohibit it as soon as possible." That was eleven years ago, and the American proposal that such imports shall

[1] BRITISH MEDICAL JOURNAL, December 6th, 1924, p. 1071, and January 10th, 1925, p. 83.

be reduced by 10 per cent. annually for ten years would not seem to be unduly expeditious. The counter-proposal put forward by the British Government is that opium smoking shall be abolished in British Far Eastern territories within fifteen years from such suppression of opium production in China as will preclude the drug being smuggled into those territories. This suggestion was stigmatized by Dr. Sze, the Chinese delegate, as "an attempt to escape from a definite obligation by subordinating it to a condition depending on the internal situation of another country," and Mr. Porter, the American delegate, could not admit that "any justification existed for even a temporary suspension of an obligation which rested upon every nation signatory to the Hague Convention."

In a recent paper read before the Royal Society of Tropical Medicine, Professor W. E. Dixon contended, in regard to smoking opium, that "this way of taking opium is by far the least objectionable, because the amount of alkaloid taken is so small." This view appears to conflict with what may be termed the official or administrative view in India, and if correct would seem to indicate that administration by way of ingestion or injection requires to be more seriously regarded and more effectively restricted than the smoking of opium. An interesting article in the *Times* of January 26th gives alarming figures of the extent of drug addiction, especially of heroin and cocaine, in the United States, and the enormous sums—estimated at over £40,000,000 a year—spent by these wretched addicts in procuring their supplies. It concludes by stating that the one certain "way of cutting off supplies is to limit the production of opium and of coca leaves in their countries of origin."

THE RIVERS MEMORIAL MEDAL.

THE council of the Royal Anthropological Institute recently determined to commemorate W. H. R. Rivers, M.D., F.R.S., who was its president at the time of his death, by founding a Rivers Medal, to be given annually for meritorious anthropological work done in the field. The first presentation of the medal took place at a meeting of the institute on January 27th, when the award was made to Dr. Alfred C. Haddon, F.R.S., reader in ethnology in the University of Cambridge, in recognition of his work in New Guinea, Torres Strait, and Borneo. The president of the institute, Professor C. G. Seligman, in presenting the medal (which has been struck by the Royal Mint and bears a bust of Dr. Rivers on the obverse), said that the names of W. H. R. Rivers and Alfred Cort Haddon were closely united in many ways. In honouring both on this occasion anthropologists were praising famous men and—for he might well carry the quotation further—"our fathers that begat us." There was no one who had added anything to the science of anthropology during the last fifteen years who was not indebted to a greater or less extent to one or other or both of these men. They worked together, and personally he had no doubt that neither of them would have achieved what he did had he worked alone. The medallist of that evening, upon whom this honour was most naturally and fittingly bestowed, was the first man to organize a really important anthropological expedition in the field. Every working anthropologist, directly or indirectly, was under obligation to the initiative behind that expedition to the Torres Strait a quarter of a century ago. The expedition proved to be the most important that had ever been organized, and it was a striking proof of Haddon's foresight that he determined that it should be not only an anthropological but a psychological expedition also, and that half of its members should be trained psychologists. The opportunity of that expedition sent the subtle spirit of Rivers roving over that fascinating "no man's land" in which ethnology, psychology, genetics, sociology, and even medicine all interpenetrated. There he experimented to some extent, but more than anything else he speculated, and speculated brilliantly. Professor Seligman added that he had had the privilege of working with Rivers in the field, and he was able to see the very distinct connexion between the genealogical method which he hammered out in Torres Strait and the work he did for shell-shocked soldiers in the war. After speaking of the work of Dr. Haddon as author and teacher, he said it was with mingled respect and affection and sorrow and pride that he presented the Rivers medal for the first time. Dr. Haddon, in acknowledgement, said that he was very happy to have his name associated with that of his old friend and colleague. After his own first expedition of 1889 he gained an appreciation of the fact that psychology was an integral part of anthropology, and he determined that if he had opportunity in this direction he would not neglect it. When he was preparing for the Torres Strait expedition in 1898 he asked Rivers to join him, and Rivers refused, but later, finding that C. S. Myers and another of his best students were going, he changed his mind and asked to be allowed to join the party. The consequence was that for the first time natives were studied psychologically in their own country by trained psychologists with adequate apparatus.

SEX RATIO AND ITS CONTROL.

THE sex ratio is the relative numerical proportion of the sexes within the group. There must be a sex ratio at all times after sex is determined, but it is convenient to take conception, birth, and maturity as the three salient stages at which to consider the matter. These points are known as the primary, secondary, and tertiary sex ratios respectively. That usually considered is the secondary, which is measured by taking the number of males to a hundred females born. The primary is a demonstration of the sex-determining mechanism in operation, while the tertiary will provide an estimate of the post-natal survival of the two sexes. That this subject is of more than academic interest is demonstrated by Dr. F. A. E. Crew in a recent paper.[1] To the dairy or egg farmer it is of supreme importance that females should preponderate in his young stock, and the question is not without its human interest. It seems probable that sex is determined at the moment of fertilization. It has been shown that there exists a mechanism tending to preserve in each generation the ratio of 1 to 1, and that this mechanism is inherent in the gametes in virtue of the production in equal numbers by one sex (the female in birds, the male in amphibians and mammals) of male and female reproducing cells respectively. Thus in mammals the female is homogametic—that is, produces ova capable of becoming either male or female; while the male is heterogametic—that is, produces sperms which are either male or female, but which, once produced, must determine the sex of the resulting embryo. The sex ratio is theoretically 1 to 1, but in practice this is never the case. Dr. Crew gives some interesting figures. In man the ratio is 103 or 107 to 100; in horses and sheep the males are in the minority; while in dogs, pigs, and cattle there are from 118 to 107 males to every 100 females. In certain animals it has been found possible, by careful in-breeding, to produce a line in which either males or females predominate. There is evidence that in man the sex ratio is low for births resulting from conceptions at the seasons of greatest fecundity and high at the times of lowest birth rate. This difference is possibly related to the relative physiological condition of the parents at the time. Statistical evidence

[1] *Inter. Rev. of Sci. and Prac. of Agric.*, Rome, ii, 1924, pp. 554-570.

sometimes supports, sometimes contradicts, the belief that the ratio is affected by the relative ages of the parents. The old notion that one ovary produces only male eggs and the other female, and that they function alternately, cannot be brought into harmony with established facts, and is not accepted by geneticists on present-day evidence. Unilateral ovariotomy does not result in the production of young of only one sex—in fact, the evidence all tends to show that in mammals the sperm always carries the sex-determining constituent. Experimentally, the sex ratio can be varied, but in nature the deviation from the theoretical number is probably most often due to a differential activity, susceptibility, or mortality on the part of the two kinds of sperm. An individual with the chromosome constitution of one sex (a determined male or female) may come to possess the functional sex equipment of the opposite. A hen, for example, may function as a cock, and when mated with a hen will provide offspring with a ratio of 1 to 2; whereas a female mammal functioning as a male will produce progeny exclusively female. The problem of the control of the sex ratio is that of the control of the differential production of the gametes by the heterogametic sex, of the control of the physical and physiological dimorphism of these, or of the production of complete sex reversal.

"RADIO" DIAGNOSIS.

THE exploitation of wireless as a means of medical charlatanry appears to be now in full swing in the United States. It is like the outburst of electric quacks that followed the discovery of magnetism and electricity. We are not surprised to learn, therefore, from the *Radio News*[1] that a new method of "radio" diagnosis has been evolved: this is the neurophonometer of one G. D. Rogers, D.C., Ph.C., a chiropractor from Texas. Since these new American contraptions have a way of crossing the Atlantic rapidly, medical practitioners in this country may find it convenient to be acquainted beforehand with the name of any new marvel that may be brought to their notice by credulous patients. The *Radio News* describes the neurophonometer as a regular radio receiving set, fitted with a headphone and two additional leads. One of these leads is attached to the forehead of the patient, while the other is applied to various sites on the patient's back. The operator wears the headphone, and tunes in the set until he hears a maximum sound. By this means he can ascertain in ten minutes the "capacity and inductance of the brain," and can determine which spinal nerves show a divergence from this inductance. Chiropraxis, as some of our readers may remember, teaches that all disease is due to compression or interference with spinal nerves, and this machine enables the chiropractor to know the nerve on which he should start operations. This all sounds delightfully simple, but the *Radio News* treats the thing in a spirit of mocking disbelief. Thus it heads its article "The latest radio swindle"; it writes of "the crass nonsense of the technical verbiage"; and it uses unkind expressions such as "hokum," "humbug," "pure bunk," and "fool the public." Our contemporary mentions that it has had occasion before to issue warnings against the crop of unscrupulous exploiters who have sprung up to take advantage of the public interest in wireless, and it compares them with burglars, to the advantage of the latter. It concludes by offering to pay Rogers 1,000 dollars if he can convince a scientific committee that his claims are true. The trouble about "inventions" of this kind is that they are so numerous and so varied. Most of them die a speedy death, but a few catch on, and it is just as hard to say which one will hold the public fancy as it is to say which pantomime song will captivate the hearts of the London errand-boys.

[1] *Radio News*, December, 1924, p. 914.

UROSCOPY.

AT a meeting of the Section of the History of Medicine of the Royal Society of Medicine, held on January 21st at the Wellcome Historical Medical Museum, Professor Gask read, on behalf of Mr. Alban Doran (who could not be present), a short communication on bed-bottles and glasses for uroscopy. Mr. Alban Doran's exhibit consisted of three bed-bottles, one for male and two for female patients. They were recently purchased in a curiosity shop in Oporto, Portugal, by Lieut.-Colonel Croft Lyons. Made of white flint glass, they appeared to be uroscopy glasses for the examination of urine, such as are figured in the works of Johannes de Ketham (a set from Leipzig University, 1400) and Fletcher (1641), and in the pictures of Gerard Dow and van den Bosch, painted in the seventeenth century, copies of which were exhibited. On close examination, however, and comparison with samples of both kinds of glasses in the Wellcome Museum, it became clear that these three bottles were made for the use of invalids in the recumbent position, and not for the preservation of urine for examination. They were known to English nurses as "bed-bottles," and were much in use down to the end of the eighteenth century in this country, and remained popular in Southern Europe until later times. The curator of the museum (Mr. C. J. S. Thompson) then showed and described a number of specimens of uroscopic glasses, dating from the fifteenth to the seventeenth centuries, now under his care. The antiquity and purpose of such glasses were discussed by Dr. J. D. Rolleston, President of the Section, Dr. Parkes Weber, and others. In mediaeval art the physician is, in a very large proportion of cases, depicted in the act of inspecting urine in a glass. As Garrison says: "Uroscopy or water-casting was, in fact, a favourite theme of the painter and wood-engraver down to the beginning of the eighteenth century, and the accessories in these representations are nearly always the same. The urinal became the emblem of medical practice in the Middle Ages." We seem to remember that one of the Florentine churches is decorated with a carving in stone, by Giotto, showing a physician in the conventional attitude examining a specimen of urine, and attended by persons carrying the usual urine basket. The meeting concluded with a very interesting display of anatomical manikins exhibited by Mr. Thompson. The bed-bottles shown by Mr. Doran are preserved in the museum of the Royal College of Surgeons of England.

THE SCIENTIFIC OUTLOOK IN VENEREAL DISEASES.

IN his address from the chair of the Medical Society for the Study of Venereal Diseases, on January 23rd, Colonel L. W. Harrison said that many clinicians worked too much on empirical lines, content to follow a path recommended by some enterprising author without any clear idea of the principles which should govern their line of action. A new situation, created, for example, by some abnormal or anomalous symptom, drove them to a book, not to discover the underlying pathological processes, but to find out what the author did in a like situation. Often also clinicians and pathologists worked in water-tight compartments. Much of the criticism levelled at the reliability of the serum tests for syphilis sprang from a lack of knowledge on the part of many clinicians of the scientific principles and technique of the tests, and a consequent inability to understand the pathologist's point of view. For example, a reaction was often—too often, he thought—described by the pathologist as "weakly positive." A clinician who had not taken the trouble to understand the test, on receiving this report, and having a suspicion already that the patient was suffering from syphilis, might seize upon the word "positive" and use it as his justification for a diagnosis,

thereby inflicting not infrequently on a non-syphilitic patient a great amount of mental suffering. A little knowledge of the technique and a little conversation with the pathologist would prevent the clinician from accepting the report of a "weakly positive" reaction as justifying anything more than a suspicion, and would make it clear to him that between the plainly syphilitic reaction and the plainly negative there was a zone into which some non-syphilitic serum now and then wandered. The pathologist, in reporting a serum as "weakly positive," never intended to convey more than a doubt, and was not in the least surprised to find the same serum a week later giving a negative reaction. Colonel Harrison went on to remind the society of a few of the ways in which laboratory work had advanced the arts of diagnosis and treatment, and said that workers in venereal diseases owed more than those in most other branches of medicine to laboratory research. Arsenobenzol compounds were largely the product of pure research. The bismuth treatment owed its existence to a surmise by a chemist that from its chemical structure bismuth should be antispirochaetal. The serum tests of syphilis were due to the laboratory, also the discovery of the micro-organisms of syphilis and gonorrhoea. Histologists had shown the toxic effects of antisyphilitic treatment, and biochemists had indicated some of the ways in which these toxic effects might be avoided. He spoke also of the value of a laboratory training to the clinician, and added that he felt keenly the debt which he himself owed to his training in laboratory methods of attacking problems. The laboratory worker had the advantage over the clinician that his observations could be more exact, he obtained his results much more quickly, and wrong processes of thinking were corrected sooner. The speaker illustrated his point from the attack made on the vaccine therapy of gonorrhoea; this attack had usually been based on the mere impression of an observer that cases treated with vaccines did no better than those treated without, but very often no attempt was made to discover exactly whether the cases treated with vaccines were under the same conditions as those not so treated, no exact computation was made of the results with regard to complications or the period of infectivity, and no thought was given to the possibility of the particular vaccine which was employed being an indifferent immunizer. The observer, in fact, generalized from his mere impression that vaccines were useless in the treatment of gonorrhoea. Certainly there would be every advantage in the closer study by the clinician of the scientific principles involved in the diagnosis and treatment of venereal diseases. The clinician who viewed clinical phenomena from the pathological standpoint developed ideas as to methods which might combat the pathological process. These ideas he took to the pathologist, and together they worked out a line of research which was more likely to be fruitful than if the clinician had simply, in some haphazard way, put his serums to the test. He pleaded that the clinician and the pathologist should each learn something of the other's work, and endeavour to understand its possibilities and limitations.

"GLÜCKSGEFÜHL."

THE *Glücksgefühl*, which is the subject of a monograph by Dr. Rümke of Amsterdam,[1] is difficult of definition. It is not the ordinary sensations of joy, pleasure, or happiness that are dependent on external excitation, nor is it the sense of physical well-being. It is essentially psychic, and arises unexpectedly and spontaneously from the innermost psyche of the individual who experiences it; it permeates his whole being, and colours all animate and inanimate objects within his horizon. It is, in fact, much of the same nature as that which Wordsworth describes in his "Intimations of Immortality": "The earth and every common sight" seems "apparelled in celestial light." This peculiar sensation of bliss is analysed by Dr. Rümke from manifestations of it amongst psychopathic and neurological patients, in the aura of epileptics, and in persons under the influence of such drugs as opium and cannabis indica. His object is to give prominence to his method of analysing phenomena in psychic conditions. He has examined the medical histories of 5,000 cases in the Valerius Psychiatric and Neurological Clinic of Amsterdam. In only nine, however, did he find material suitable for his investigations. He discusses these cases in detail, recording first the phenomena in each case, and in later pages of the book the family and medical history of the patient. He also describes and analyses nine cases reported by other writers as occurring in conditions of neurasthenia, epilepsy, mental disease, and opium intoxication, contrasting them with the cases from the Valerius Clinic. The monograph chiefly consists of a meticulous examination of the manifestations of *Glücksgefühl* in all these cases and in all their aspects and bearings. A wide German and French literature comes within the scope of Dr. Rümke's investigations. It includes, for example, the metaphysical and sociological writings of Schopenhauer and Nietzsche. The only works in English referred to are McDougall's *Introduction to Social Psychology* and James's *Varieties of Religious Experience*. In his concluding remarks Dr. Rümke emphasizes the value and possible future development of a study of the phenomena from a prognostic point of view, but states that clinically, etiologically, and therapeutically his method leads to nothing; and, indeed, to use his own expression, might appear to be a process of "splitting hairs." But patients who have experienced *Glücksgefühl* may, when the phenomena present certain favourable characteristics, recover their sanity and remain sane for years, while those with unfavourable characteristics do not. It is in the differentiation between the favourable and unfavourable manifestations that Dr. Rümke believes his method of investigating phenomena is of value. When its limitations are recognized it becomes, therefore, an auxiliary to other clinical methods in psychiatry because of its value in prognosis.

[1] *Zur Phänomenologie und Klinik des Glücksgefühls.* By Dr. H. C. Rümke. Berlin: Julius Springer. 1924. (Sup. roy. 8vo, pp. 28. 1.45 dols.)

JOSEPH ROLLET.

ON the occasion of the celebration of the centenary of Joseph Rollet, who was born on November 24th, 1824,[1] Dr. L. Bonnet, in a special issue of the *Lyon Médical* published on December 7th, 1924, recalls that Rollet had originally intended to devote himself to surgery, but that on his appointment as surgeon-major to the Antiquaille Hospital at Lyons he determined to concentrate his attention on venereal disease. The introduction to his book published in 1861, entitled *Recherches cliniques et expérimentales sur la syphilis, le chancre simple et la blennorrhagie*, is printed in the same number of the journal. In it Rollet pointed out that the historical method employed by Astruc and the experimental method of inoculation introduced by John Hunter are not the only guides in the investigation of venereal disease, but may be compared to roundabout paths for scaling a mountain inaccessible by a direct route. In the majority of cases, he maintains, there is nothing to take the place of the good old method of clinical observation, whether in general medicine or in the domain of venereal diseases. At the time of Rollet's appointment to the Antiquaille Hospital in 1855, almost as much confusion reigned in the nosology of venereal disease as in the subject of sore throat and continued fever before the time of Bretonneau.[2] Rollet's principal achievement is to have established once and for all the absolute independence of the three venereal

[1] See BRITISH MEDICAL JOURNAL, November 22nd, 1924, p. 981.
[2] See BRITISH MEDICAL JOURNAL, November 1st, 1924, p. 824.

diseases. He showed that those cases in which an apparently soft chancre was followed by constitutional syphilis there was not a mysterious transformation of one virus into another, but an association of the two viruses. Rollet's next most important work was the demonstration of the contagious character of the lesions of secondary syphilis, for he held that it is by this means that the disease is commonly transmitted. He was thus able to recognize and describe several new varieties of chancre caused by these lesions, such as mammary chancre, buccal and cephalic chancre, and the chancre of vaccinal syphilis. Rollet's other contributions to our knowledge of venereal diseases consisted in his demonstration of the syphilitic nature of that form of sarcocele known as fungus testis, his work on gonorrhoeal rheumatism, and the early and masked forms of urethral stricture.

ROYAL COMMISSION ON LUNACY AND MENTAL DISORDER.

(*Continued from page 172.*)

THE Royal Commission resumed its sittings on January 26th, the Right Hon. H. P. MACMILLAN, K.C., presiding.

Allegations of Wrongful Detention.

The case of Mr. H. W. Holman, who at the previous sitting had alleged that he had been wrongfully committed as a lunatic to a mental hospital, was further investigated. Mr. H. C. Dickens, K.C. (instructed by the Medical Defence Union), appeared on behalf of five medical men who had been mentioned in Mr. Holman's evidence, and the medical witnesses were cross-examined by Mr. W. Stewart, secretary of the National Society for Lunacy Reform.

The Chairman at the outset said that Mr. Holman had addressed a most improper letter to the Commissioners reflecting on a medical witness. A court of law would take serious notice of such communications. He added that the Commission only desired to know why it was thought proper in Mr. Holman's case to issue an urgency order instead of the more deliberate reception order, and also to learn something of the circumstances of the certification, Mr. Holman having alleged that his examination before certification was insufficient and careless.

Sir Maurice Craig said that he first saw Mr. Holman in January, 1915, in consultation with his own doctor. He was stated to have been for two years very excitable and talkative, and once to have tried to throw himself out of an hotel window. He had delusions also that he was liable to arrest as a spy. He wandered from one subject to another in conversation, his pupils did not react well to light, and his knee-jerks were sluggish and unequal. Among other signs was a high lymphocytosis—about 86 per cent.—indicating something pathological mentally. He also indicated other tests which to his mind afforded a clue as to the condition from which the patient was suffering. As to the reason for an urgency order, the patient was at that time at the house of a doctor (Dr. Dempster), and there was no power of restraint. Mr. Holman threatened to leave, and the likelihood was that had he done so he would have got into the hands of the police, as he was not then in a fit state to take care of himself. The witness had tried to avoid sending the patient to a mental hospital by getting him to sign an undertaking to carry out certain promises, but this was broken. He asked Dr. Percy Smith to see the patient, and that gentleman had a long interview with him alone. What he (Sir Maurice) saw and knew of the patient certainly justified his detention.

After Mr. Stewart's cross-examination of the witness had proceeded for some little time, the Chairman expressed regret that the opportunities which Mr. Stewart was enjoying in cross-examining the witness were being largely wasted by irrelevant issues, apart from the loss of valuable time to many people concerned.

Mr. C. R. Steel, a solicitor, spoke to visiting Mr. Holman while he was detained at Moorcroft and executing a codicil revoking his will. He did not think Mr. Holman was of unsound mind, but he took no steps to procure his release, as he did not want to set his opinion against that of the doctors.

Dr. W. H. B. Stoddart said that he first came in contact with Mr. Holman in 1916, and he produced the notes he made of his condition at the time, taken at the home of Dr. Dempster. The question was whether he was in a condition to go unaccompanied to his home at Brighton, as he intended doing, and the witness considered that he should not go anywhere without proper supervision and control. The urgency order was rendered necessary by the patient's determination to leave and the fact that there then existed no power to detain him. The witness was cross-examined as to his recently modified views in favour of Freudism, and he admitted that he now looked at the pathology of mental disorders from a somewhat different angle. But this did not change his opinions on the question whether a given patient was certifiable. He added that there were impressions derived from a long interview with a patient which could not be adequately represented on the form of certificate. He denied that he certified because he was told to do so; no doctor would do that. He was asked for his opinion and gave it very firmly.

On the following day, January 27th, the case of Mr. Holman was further investigated.

Dr. Percy Smith, examined by Mr. Dickens, said that he saw Mr. Holman in December, 1916, and from information received from three doctors and his own examination of the patient he came to the conclusion that he was insane and unfit to remain in the doctor's house, where he then was. In cross-examination by Mr. Stewart, the witness said that he could not tell how many patients he certified during 1916, but the patients he saw and did not certify were far more numerous than those he certified. He would do anything in his power to avoid certification so long as it was safe. Before seeing the patient he had a long talk with Sir Maurice Craig. He did not warn the patient that anything he said might be used as material to justify his detention; to do so in many cases would be extremely detrimental.

Mr. Stewart at this point read a judgement of Lord Justice Coleridge in 1885, in which he spoke of "the flimsy stuff on which people are sometimes incarcerated in lunatic asylums." The Chairman said that that was previous to the Act of 1890, in which certain reforms were embodied, and what the Commission was now considering was whether the existing code was sufficient. Matters before 1890 were based on a very different régime. Dr. Percy Smith remarked that he was medical superintendent at Bethlem when the Act of 1890 was passed, and well remembered that the Act trebled all the certificates and reports a medical superintendent had to make with regard to patients of the private class. Mr. Stewart maintained that the Act of 1890 was a consolidating Act, and, save in one particular, brought about no alteration in procedure.

Dr. Percy Smith, in answer to a further question, whether, in the light of what was now known about Mr. Holman's case, he wished to express any regret that he had certified the patient, said that he had no regret at all. The Chairman said that if Mr. Stewart proceeded along such a line of cross-examination he would feel called upon to read communications—very serious communications—which had been received from Mr. Holman subsequent to his evidence, including a telegram received that morning.

Dr. Porter Phillips, medical superintendent of Bethlem Hospital, described how he was asked by Sir Maurice Craig to see the patient, and found him in a restless, excited, and talkative condition. He had great difficulty in questioning him because of his garrulity. He noticed unequal pupils, exaggerated reflexes, and a very tremulous condition. He came to the conclusion that Mr. Holman was of unsound mind. Mr. Holman took a fancy to the witness, and expressed his willingness to go with him to a home he had described with a view to treatment; and after consultation with Dr. Dempster, in whose house he was, it was agreed that advantage should be taken of the opportunity afforded by the patient's willingness to be removed, and the witness himself drove him to Moorcroft and left him there. He strongly denied that there was anything in the nature of a "trap."

After further evidence the Chairman announced that the Commission had heard sufficient of the case of Mr. Holman, and there was no need to call Dr. R. H. Cole, who was present and ready to give evidence.

National Society for Lunacy Reform.

Evidence was then taken from Mr. Montgomery Parker, chairman of the National Society for Lunacy Reform.

Mr. Parker submitted, on behalf of his society, that there should be an immediate provision of voluntary hostels for early treatment supported by subsidy from the State and controlled by the Minister of Health through local health authorities, with no association with the Board of Control or any other board or department of State control of lunacy administration. There should be early adaptation of hospital wards to deal with incipient mental cases or temporary cases arising from illnesses with temperature, and also adaptation of infirmary wards to permit of better classification and observation. On the legal side there should be unification of the methods of certification and the issue of reception orders; the elimination of all unnecessary distinctions between private and pauper patients; no special fees to be payable to permanent officers functioning prior to certification; provisions to secure that the judicial authority should in all matters act judicially and not in an administrative capacity; adequate representation of the patient at all stages, and some further definition of the terms "lunatic" and "of unsound mind." With regard to institutional treatment the society submitted that it was desirable to establish a new and more representative central authority, with definite powers of direction, acting through assistant district commissioners, who should be trained administrators and not medical men. It laid stress on the urgent necessity of applying generally improved methods of curative treatment and improving the general environment of patients, of limiting where possible the numbers permitted in any one institution, of increasing the medical and nursing staff to permit of proper treatment, and of strengthening existing visiting committees with such provisions as would ensure their functioning, of a statutory provision that all institutions should appoint a body of authorized visitors independent of lunacy administration and licensed as in the case of prison visitors, the introduction of the judicial authority within asylums as an additional safeguard to the patients and the right of independent medical access to patients. There should also, in the society's opinion, be some provision that the senior medical officer should not be charged with responsibility for administrative duties. Other points set out were that there should be no punishment under the guise of "curative treatment," that there should be further provision to ensure that the existing methods of discharge were freely utilized in practice, and that after discharge there should be extended opportunity for after-treatment.

England and Wales.

NOTTINGHAM GENERAL HOSPITAL.

MR. W. G. PLAYER of Nottingham, a past-president of the Nottingham General Hospital, and at present vice-chairman of the Hospital Board and chairman of the Extension Committee, has promised a subscription of £50,000 towards the capital sum required for the extension of this institution. Mr. Player had previously given £10,000 to the War Memorial Extension Fund of the hospital, and also endowed five beds at the cost of £5,000. The present subscription follows the lines of a scheme devised by the National Council of Social Service, under which charities are exempt from income tax in respect of income applied to charitable purposes. It is possible, therefore, that Mr. Player's subscription will ultimately result in as much as £70,000 accruing to the hospital. Great advances have been made since 1920; the income of the hospital has considerably increased, the overdraft at the bank has been extinguished, and the new nurses' home, erected as a war memorial, was opened by the Prince of Wales in 1923. We gave an account of this building on August 25th, 1923 (p. 339). In consequence of Mr. Player's gift it may now be possible to provide for the completion of the Ropewalk Wing of the hospital, and also, it is hoped, for the erection subsequently of the much needed wing for accident cases. The new out-patients' department, now being built, will cost about £80,000, and will contain a large central hall for out-patients, consulting rooms, an operating theatre, forty beds for throat, nose, and ear patients, orthopaedic wards, and *x*-ray and massage departments. It will be connected with the main hospital building by a subway. The estimated annual cost for maintenance of the new department will be approximately £5,000. As soon as it has been completed it is intended to build a new wing of the hospital containing 120 beds, which will increase the cost of maintenance by £10,000. During 1924 the Nottingham General Hospital received from the Hospital Saturday Fund over £24,000. Since 1919 the Hospital Saturday Fund has increased its contributions by £16,000, and the annual subscriptions have increased by £6,000. The total maintenance cost has risen from £20,000 per annum, prior to the war, to approximately £50,000 for 1924. The 235 beds available in 1913 have increased to 350. Some 5,000 in-patients are admitted annually, and 27,000 out-patients receive treatment. Accident and emergency cases have increased during the past ten years by more than 100 per cent. The oldest portion of the hospital was erected in 1782, and contains 44 beds. The hospital, which serves an area containing a population of more than half a million, will no doubt be visited by many of our readers when the British Medical Association meets at Nottingham in 1926.

PROMOTION OF MEDICAL STAFF AT LONDON MENTAL HOSPITALS.

In 1919 the London County Council agreed to the principle of granting study leave to members of the medical staff at the mental hospitals, and the Mental Hospitals Committee also at that time laid it down that in making appointments to positions in the medical service special consideration should be given to candidates holding degrees in psychological medicine. The committee is now recommending to the Council that the promotion of members of the medical staff, other than those who were in the service on July 8th, 1919, to positions higher than that of fifth assistant medical officer, should be conditional upon the officer promoted possessing a diploma or degree in psychological medicine or obtaining such a diploma or degree within a limit of time to be specified by the committee.

COMBE PARK ORTHOPAEDIC HOSPITAL, BATH.

Descriptions of the Combe Park Children's Orthopaedic Hospital at Bath were given in our issues of May 24th and July 26th, 1924. At a recent meeting of the General Committee of the hospital representatives of the Wiltshire County Council and clinics attended to discuss a scheme whereby arrangements could be made for the treatment of the crippled children of Wiltshire. The Marquess of Bath presided, and, as the result of discussion, it was agreed to recommend that a board of governors be formed, including representatives of the Wiltshire County Council and local clinics. Since this meeting was held Somersetshire has also decided to take part in the scheme, so that eventually there will be one central orthopaedic hospital for the whole of Bath, Somerset, and Wilts, with clinics for the after-care of those patients who have been treated at the central hospital. The increased accommodation thus necessitated is receiving attention.

THE INTERNATIONAL HEALTH CONGRESS AT WEMBLEY.

We gave an account in our issue of June 7th, 1924 (p. 1013), of the first International Health Conference organized by the People's League of Health at Wembley last May. The Conference dealt with "the latest knowledge regarding the causes, treatment, and prevention of disease," and a full report has now been published. The text of the various papers read, and of the discussion, will enable those who were present to recall the valuable information that was so lucidly set before them. The subjects dealt with were: housing and house sanitation; food, air, light, and water; some overseas and other diseases; mental hygiene; industrial hygiene; and legislation desirable with regard to health matters. Sir Clifford Allbutt contributes a foreword in which he indicates the value of the work accomplished by the People's League of Health. With regard to the evils characteristic of our civilization, he suggests that it is satisfactory to learn from the Conference that our embarrassment arises less from the number of arrears to be made up than from the continual rise in our standards. The report is published by the People's League of Health, and is obtainable at its headquarters, 12, Stratford Place, W.1, price 7s. 6d.

The League has arranged a course of nine lectures on diet, to be given during the present term at the house of the Medical Society of London, 11, Chandos Street, Cavendish Square, W.1, at 6 p.m. on each day. The first was given on Wednesday last by Dr. Leonard Hill, F.R.S., on the nature and purpose of food; the second, by Lieut.-Colonel R. McCarrison, C.I.E., M.D., on the food of mankind treated historically and geographically, will be given on Wednesday, February 4th. On the three following Wednesdays Professor R. H. A. Plimmer, D.Sc., will lecture on diseases caused by badly chosen diets.

Scotland.

EDINBURGH ALUMNI ASSOCIATION.

A MEETING was held in the McEwan Hall, Edinburgh University, on January 23rd in connexion with the recently formed Edinburgh University Alumni Association. Sir Harold Stiles, president of the association, occupied the chair, and the Earl of Balfour, K.G., O.M., Chancellor of the University, was the principal speaker. The chairman at the outset stated that the suggestion for the formation of an Alumni Association had been made about a year ago in consequence of the great benefits which had resulted from the founding of similar associations in America. Up to the present time 842 graduates had joined the association, and it had received subscriptions to the amount of £1,075. The latest gift, received that day, had been one of £1,000 from Dr. Charles M. Cooper (San Francisco), of which the yearly interest was to be paid to some assistant in the University Medical Department in order that he might make himself familiar with different lines of medical research and act as practical adviser to young graduates and undergraduates who desired to do some research work. Lord Balfour said that he rejoiced to have an opportunity to address representatives of the University with which he was so closely connected on a subject which he was convinced was going to have a growing success. It seemed to him that they were too often inclined to consider a university as merely a mechanism for imparting a certain amount of learning and for giving an equipment for future success in life to the student. That, no doubt, was its primary object and one of the greatest purposes a university could fulfil. If it did not fulfil that object, certainly it would not deserve the name of a great

centre of education; but he thought that another general proposition ought to be admitted—that a man who extracted only this amount from his university training was one of the most unfortunate children of that university. A university ought to give much more, and it habitually gave much more, than those bare accomplishments which could be tested by examinations. Anyone who had been through a university would find that after all it was the human intercourse that accompanied instruction, the intercourse between pupil and teacher and the intercourse, at least as much, between fellow pupils, the sensations of union, patriotism, and love of the community of which for some years they were members, that combined, and ought to combine, into the feeling inseparable from each man's view of his own career, and inseparable from love of country and love of home, to help him to serve all the causes to which in the course of his life he might find himself attached. Could it be doubted that if an institution like that, which had had the most admirable effects in the United States, were introduced in Edinburgh, it would be doing a great work for the present and future of the University? Members of the University of Edinburgh were drawn from all parts of the globe for their education, and again scattered to all parts of the world, and carried with them some fragments of those higher things that they had learned within its walls. If an organism were given to these men scattered all over the habitable globe—a living organism which would be a bond of union between those who had left the University and also between them and those who were still either students or teachers carrying on the work of the University—it would be a great advantage to those institutions in their country to which the higher life of the country owed a great debt. That was the main object of the society which had been founded. Another object was the money side of the question; no large sums were to be asked of any member of the society. In fact, the sum asked for was deliberately kept within the resources of those whose means were very small, although he hoped that the obligatory sum would in many cases be exceeded, and that those who were prosperous among the alumni would not be slow to remember the source from which, without doubt, they obtained much of the strength, knowledge, and training to which their success was due. He laid stress on this because he was convinced that even now the general public of this country did not fully appreciate the needs from which the universities inevitably suffered. The cost of teaching had greatly increased, and the cost of research had been added to it in every university worthy of the name. Literature, mathematics, history, philosophy, and theology could be taught without any great cost of apparatus, although it was necessary now to have buildings adequate to the dignity and importance of the work which the university had to accomplish, and such buildings could not easily or cheaply be obtained. When, however, they turned to the experimental sciences and to medicine the cost of teaching was found to be inevitably growing, and, so far as he could see, was inevitably going to grow. These things could not be done cheaply, and without pecuniary assistance modern research and teaching could not be effectively carried on. So much change had taken place in science that even the language used by physicists in 1925 had no resemblance to the language used by physicists thirty years ago. The progress had been revolutionary, and he believed that those who could speak with more authority than himself in regard to medicine would say that, though the changes had not been so revolutionary as the changes in physics, they had nevertheless been of an astounding character, and from the mere fact of their magnitude required an immense volume of merely material assistance. He confessed that he envied their friends on the other side of the Atlantic, who appeared to have an unlimited number of millionaires, prepared to spend their millions, and to come forward time after time to help the good cause of scientific research. He wanted everyone who heard him to feel that this monetary task could not be wholly accomplished either from existing endowments of universities, contributions of Governments, the help of local authorities, or by students' fees. The University had the right to ask those who were not millionaires, and especially those who in their time had gained all that the University had to give them, to contribute according to their means to the task whose outlines he had endeavoured to draw. Sir Alfred Ewing, Principal of the University, in moving a vote of thanks to Lord Balfour, said it appeared to be increasingly the case that the main concern of a principal of a university was with finance—the getting of money and the spending, or rather the not spending, of it. In his pessimistic moments he thought of the University as a big balance sheet, or as a sheet which would never balance, and it was a relief to hear Lord Balfour say that after all the University was an organism with a soul.

Reduction of Scarlet Fever.

A circular has been issued by Dr. W. Robertson, M.O.H. for Edinburgh, to practitioners in that city, pointing out that the cost of treating cases of scarlet fever last year in the Municipal Hospital was £20,000. Of the cases notified 92 per cent. have received institution treatment. He suggests that the infection of scarlet fever can be controlled with comparative ease, if contact between infected and non-infected persons is avoided, and if inunction and swabbing (Milne treatment method) be adopted as a preventive. As a result of his previous experience in Leith, he maintains that when this form of treatment was employed the numbers admitted to hospital fell from 83 per cent. to 65 per cent., and he tells of a series of 200 consecutive cases of scarlet fever treated in their own homes in Leith, the largest proportion of which were working men's dwellings, and among which not a single instance of spread of infection took place among other members of the different families involved. The instructions given for this form of home treatment are to swab the throat with carbolized glycerin (1 in 10) every two hours for the first twenty-four hours, and for the first four days to anoint the whole surface of the body twice daily with ordinary eucalyptus oil (undiluted); thereafter the anointing is continued once daily for the ensuing ten days.

Human Nature through the Doctor's Eyes.

A popular lecture with this title was delivered on January 6th, under the auspices of the Philosophical Institution, in the United Free Assembly Hall, Edinburgh, by Dr. Habberton Lulham; the chair was occupied by Lord Sands. Dr. Lulham spoke of the attitude of the general public towards the doctor, enlivening his discourse with amusing anecdotes. He said that if a doctor were to allow himself to be emotionally affected by all the distressing things he saw, he would soon become a nervous wreck. What he had to do was to convert pity into help. The regrettable thing about cases in which nerves were not under control was that nowadays every little condition of nervousness, lassitude, or self-indulgence in its many forms was known as neurasthenia. As a means of getting rid of slight disorders, he firmly believed in making people laugh at themselves. It might be rather a mirthless laugh to start with, but if they would persevere they would find the method had a wonderful effect. He did not believe that nerves out of control were due so much to the rush and hurry of modern life as they were to the increased ease and luxury which people now demanded. He was afraid people did not stand adverse circumstances nearly so well as they used to do. The reason was that they did not make themselves stand the minor troubles enough, without running off for sedatives. Whatever else might be wrong with neurasthenics, there was too much ego in their cosmos.

Edinburgh Experiment in the Housing Problem.

An inspection was made on January 13th by the Lord Provost, Sir William Sleigh, and other members of the Edinburgh Town Council of a group of new houses that are being erected on the north side of Edinburgh in Clark Avenue. These constitute a housing scheme on community lines, the City Corporation acquiring and preparing a site for the houses, finding the money, partly from the Government subsidy, to pay the cost of their erection, and giving possession to the tenants. The latter are required to contribute an initial sum of £100 and thereafter to make annual payments of about £30, representing interest and

instalments of capital, for a period of twenty years. After this the house becomes the tenant's absolute property and the title-deeds are handed over to him. By this arrangement the municipality assumes responsibility until the payments are completed, and thus a considerable amount of expenditure in respect of titles, bonds, and capital is saved. The houses consist of five rooms with kitchen and bathroom, and cost, including the price and preparation of site, about £650. Of this amount, £75 is contributed by the Government, £50 by the Corporation, and the remainder through deferred payments by the tenant and ultimate proprietor.

NEEDS OF SCOTTISH HOSPITALS.

(*Continued from page 190.*)

THE Committee appointed by the Scottish Board of Health to inquire into the inadequacy of the hospital service in Scotland resumed its sittings on January 20th at the offices of the Scottish Board of Health, under the chairmanship of Lord MACKENZIE.

Mr. JAMES THOMSON gave evidence on behalf of the Glasgow Royal Maternity and Women's Hospital. He said that in the Glasgow Royal Maternity Hospital there were 114 beds, and they had had in the hospital as many as 170 cases at one time, while the average for the last year was 135. They had not refused any cases, but the hospital was certainly overcrowded. A great many more young mothers were thrown into hospital than would otherwise have been the case on account of the housing shortage. They hoped to make better provision by an addition which would add about forty or fifty beds. He thought that the normal cases might be dealt with through the establishment of another hospital in the town, and the maternity hospital might occupy more or less a consultant position. He did not think that patients should pay at the Maternity Hospital, for paying patients could be better treated in small homes. The Glasgow Royal Maternity Hospital was supported voluntarily, and workers' contributions amounted last year to £3,086. If the State chose to give the hospital a payment per case, the managers would have no objection to receiving it.

Professor JOHN GLAISTER (Glasgow), who for twenty-four years has been on the board of the Glasgow Royal Infirmary, next gave evidence. He said that at the present time and in view of existing legislation no provision required to be made for the well-to-do classes nor for those who were designated under the Poor Law the poorer classes. The chief demand for hospital facilities came from the so-called working class, and one of the chief reasons for this was the lessened reluctance of persons to enter hospitals. During his fifty years of association with medicine, as a student and practitioner, he had seen the desire for hospital treatment, and especially for surgical operations, develop greatly. Accommodation in Glasgow fell short of the desires and needs of claimants for treatment, and if a large new general hospital should be established its beds would be quickly filled. The present number of beds at the disposal of persons requiring urgent treatment nearly met the needs of the city, and he thought that the pressure in the ordinary voluntary hospitals could be relieved by the establishment of auxiliary hospitals and by the greater utilization of convalescent homes connected with the large infirmaries. A scheme had been in contemplation for the institution of an auxiliary hospital as a private venture at a cost of about £80,000, and an option had been obtained for a site not more than four or five miles from the city, but it was proposed to wait meantime and see what would develop. He thought that the consensus of opinion in regard to the Royal Infirmary was against the policy of admitting paying patients into the wards. During 1923, as in previous years, the bed accommodation of the Royal Infirmary had been taxed to its fullest, and the number of patients had always exceeded the number fixed for healthiest occupancy. There was no ground left within the present Infirmary boundaries for further extension. He thought that more provision than hitherto should be provided by local authorities of large populous centres for cases of surgical tuberculosis, and these, if possible, should be sent to hospitals situated in a country area. He thought also that employers and approved societies might undertake some work with special reference to occupational disablements, and an institution fitted with mechano-therapeutic appliances, electric baths, radiant heat applicators, and the like would prove a great help in expediting the recovery of workers who had received injuries in the course of their employment. With regard to Poor Law hospitals, he thought that the time had come to utilize whenever possible the accommodation available in hospitals still under the Poor Law for the needs of the general community. He could not see any objection to grants being given from State funds towards helping voluntary hospitals in difficulties, provided that the State would demand only such supervision of the accounts as would enable it to see that money so given was properly expended, and he did not think that the State should ask for representation on the boards of such institutions. He thought also that, seeing the voluntary hospitals admitted for treatment insurance patients, voluntary contributions from insurance funds would be welcomed from approved societies; and there could not be a better way of expending profits, intended by the Insurance Acts to provide additional benefits to the insured, than by annual contributions to hospitals, graduated in amount according to the benefits received by members of these societies from hospital treatment. He believed strongly in voluntary effort in the support of general hospitals, and the working classes of the West of Scotland had, as the result of systematized and continuous appeals, responded well in contributions. With regard to the amount of hospital accommodation required in Glasgow, he said that it would be necessary to depart from the old standard of one bed per 1,000, as it seemed to him that one bed in 600 or 800 was the proper estimate for a population like that of Glasgow. He believed that they still had untapped sources for obtaining voluntary contributions.

Sheriff G. L. CROLE, K.C., one of the managers of the Edinburgh Royal Infirmary, said that as regarded that hospital the number on the waiting lists for last year gave an average of 1,812 cases, although 45 per cent. of these were cases on the list of the ear, nose, and throat department awaiting the removal of adenoids and tonsils. Even with this deduction, the numbers remained higher than the managers cared to see, though it should be kept in mind that the Royal Infirmary of Edinburgh might be regarded as a national rather than a local hospital, since practically 50 per cent. of the patients in the wards came from outside the city. With regard to auxiliary hospitals, the Royal Infirmary was fortunate in having provided for it the Astley-Ainslie Institution, a first-class hospital of this type. At present only 34 beds were available, though the accommodation would increase. With regard to the other type of auxiliary hospital, where the simple forms of operation and treatment for a district could be carried out, the position presented difficulties, professional staffing being the chief of these. This, however, could be arranged by having a competent visiting staff. He was of opinion that it would be disastrous to disturb the system on which all the large hospitals had hitherto been maintained. The opportunity afforded to all classes of the community of aiding in the work by contributing according to their means must be regarded as a national asset. When he joined the board of this hospital fifteen years ago the employees in public works contributed only £8,345, while for the past year the amount that was so subscribed totalled £37,950. He did not think that any system of State aid would work satisfactorily alongside the voluntary system so far as Scotland was concerned. The time to ask a man for contributions was while he was well and working, not when he was ill with income reduced. Continuing, he said there was no doubt that the provision for maternity cases in Edinburgh was not adequate, and he thought a new maternity hospital was urgently required, while the old maternity hospital was conveniently situated for being a child welfare centre. He thought that the new maternity hospital should be in close proximity to the Infirmary. He thought that Craigleith Poorhouse might be transferred to the public health authority as a hospital auxiliary to the Royal Infirmary, and if this were taken over it would, with the Astley-Ainslie Institution, provide ample accommodation for all sick persons in the city.

Mr. A. K. RODGER and Mr. GEORGE LAIDLAW jointly gave evidence on behalf of the Victoria Infirmary, Glasgow. They said that while this Infirmary had 260 beds there was a waiting list of 333, but they contemplated building an addition to the Infirmary which would pass through about 1,500 patients each year. It was suggested that one of the ways in which the State should contribute towards voluntary hospitals would be to allow sums given voluntarily to hospitals to be free from income tax and death duties.

On January 21st the Committee took evidence mainly in relation to hospitals designed for the treatment of children. A statement had been prepared by Mr. Robert Barclay, honorary secretary and one of the directors of the Royal Hospital for Sick Children at Glasgow, which was described as the largest hospital devoted to children in the United Kingdom.

Mr. BARCLAY stated that there were now three hospitals in Scotland—at Glasgow, Edinburgh, and Aberdeen—entirely devoted to children. The Royal Hospital for Sick Children in Glasgow dealt with children up to the age of 13 years, and had accommodation for 275 patients. Extensions and additions were in progress which would make up the available cots to 350, and would provide a new out-patient department, an orthopaedic department, an enlarged electrical department, a milk department, and a department for the manufacture of medical and surgical appliances. The accommodation at the convalescent branch was to be doubled. The cost of these schemes was put at over £100,000, and an appeal, which had been issued four months ago, had resulted in the whole sum being subscribed with the exception of a few thousand pounds. The importance of the housing question in relation to children was adverted to, and this witness considered that improvement in this direction would reduce the supply of patients. The continuation of the voluntary principle was advocated, and he thought that, subject to a little rivalry, which was healthy, it was right that the large hospitals in a city should co-operate with one another. With regard to State grants, he thought that these should be apportioned between England and Scotland according to population, and not according to financial strain; and he thought that additional State aid might be given in respect of teaching and research work, which was national and not local in character, and which would not compromise the voluntary principle. He thought that before further hospital accommodation was required suitable provision should be made outside the hospitals for chronic cases, incurables, and cases of tuberculous disease, which were apt to occupy hospital beds unduly. Referring to the English method of treating paying patients alongside non-paying patients, the witness said that this was not desirable in Scotland, and could not be done without causing serious difficulties. He was, however, in favour of paying wings in hospitals, provided the paying part was quite distinct and that it could finance itself. The witness referred to the question of milk supply, which he said was closely allied to

tuberculous cases. It was vital to children that the supply should be pure, plentiful, and cheap, for at present it was undoubtedly the source of tuberculous infection, and its distribution was far from hygienic. He thought that the whole milk trade should be under the careful and systematic supervision of the Health Department or the local authorities; that the sale of milk should not be competent except from a licensed source and by a licensed dealer, and that dairies should be under central supervision.

Mr. W. J. GIBSON, chairman of the managers of Falkirk Infirmary, gave evidence with regard to hospital accommodation in the Falkirk district. They had at one time taken in patients on a paying basis, but this had to be abandoned because of the great numbers of ordinary patients demanding admission. They were, however, convinced that it would be desirable to try and provide private accommodation, for which a considerable number of people would be willing to pay. He thought there ought to be some curtailment of the present overlapping of appeals for subscriptions, for while their district contributed £3,000 yearly to the infirmaries of Edinburgh and Glasgow, the local hospital received only about £5,000, and he regarded these sums as disproportionate.

Mr. THOMSON, chairman of the Stirling Infirmary managers, stated that they were aiming at building an entirely new infirmary at a cost of £70,000, and in five weeks they had got £26,000, which showed a great interest on the part of the people of Stirling and district. He was confident that the amount required would be raised by voluntary effort, and, in this connexion, he was in favour of decentralization, and thought that more cases should be treated in Stirling instead of being sent to Edinburgh and Glasgow. He did not approve of taking money from the rates, especially in view of the conditions that prevailed just now with regard to voluntary assistance. There was no provision at present in Stirling for maternity cases, but the managers of the Infirmary intended to make provision for this purpose.

The Rev. Dr. MONTGOMERY CAMPBELL, representing Dumfries and Galloway Royal Infirmary, considered that the voluntary system ought to be continued, encouraged, and developed, and in the district of Dumfries it had amply met the necessities of the case. At the present time, however, there was an insufficient supply of beds, and, while there were nominally 113 beds in the Dumfries Infirmary, the average number of patients constantly in hospital during the last year had been 106. It was urged that immediate steps should be taken to provide more beds and the appropriate staff accommodation. He believed, as the result of long experience as a parish minister, that an element in human nature was evoked by service in the present type of voluntary hospital which would not be evoked by another class of hospital to the same extent.

The Rev. W. S. MATHIESON, representing the managers of Galashiels Cottage Hospital, said that this hospital was originally intended to meet the needs of Galashiels, but in the last few years cases had been taken in from surrounding towns, and they would have to provide increased accommodation and consider other extensions.

At the sitting of the Committee on January 23rd a plan was submitted by Mr. W. E. Whyte, district clerk, Hamilton, which would allow for State aid without State control. The memorandum runs to 39 printed pages, and refers particularly to the hospital needs of the county of Lanark, a district which, the author of the memorandum pointed out, is the most important in Scotland so far as population and valuation are concerned.

According to Mr. WHYTE's evidence, over 70 per cent. of the houses in the district are one or two apartment houses, and the housing conditions are described as so deplorable and inadequate that institutional provision for the treatment of most diseases is obviously necessary. He considered that real co-operation between local authorities was seldom practicable under present conditions, and he had in view the extension of the functions of the public health authority, but he would not advocate the granting of power to public health authorities by legislation for the establishment of hospitals for the treatment of general diseases. He would, however, advocate co-operation between the rate-aided authorities and voluntary authorities. As the capital cost of the additional institution necessary would be very great, he thought the State should set aside a sum from which capital grants to approved institutions would be made, such fund to be under the direction and control of a central body. For dealing with tuberculosis he considered there were two ways in which children might be treated: either by an extension of the Hairmyres Colony, which at present existed, or by the introduction of the Grancher system, which had been working for some twenty years in Paris, and had consisted in placing out in healthy rural families children whose health was jeopardized through living with tuberculous members of their own family. In Paris, out of 2,300 children so placed out, only 7 had developed tuberculosis. The witness thought that the voluntary system should be maintained at present, but whenever the need was such that the voluntary system could not cope with it it was the manifest duty on the part of the State to make good the deficiency. This could be ascertained by obtaining periodical returns from the various institutions.

Lady SUSAN GILMOUR gave evidence on behalf of the council of Queen Victoria's Jubilee Institute for Nurses (Scottish Branch). She said that 600 additional nurses would be required to establish a complete home nursing service for Scotland. This would make a total of 1,500 nurses so employed, and the provision was entirely a question of finance. At present voluntary contributions amounted to £120,000, and a full service could be provided by an annual grant of 6d. a head of the population of Scotland, which would yield £122,000. Both the preventive and the curative work of a nurse, who went about in the homes of the people, restricted the development of illness, and consequently the necessity for hospital treatment. The possibility of after-care in the home, which was given by adequate nursing, also shortened the time necessary for the occupation of hospital beds. A proper system of co-ordination between hospitals and local nurses by means of cards indicating treatment required would undoubtedly save pressure on hospital accommodation. About 20 per cent. of the cases attended by the Queen's Nurses throughout Scotland were treated in lieu of hospital accommodation. With a full nursing service for the whole country at least 160,000 cases would be nursed in the course of a year, and from these figures it would be seen that under the scheme suggested there would be an annual saving of hospital accommodation for 32,000 cases. The witness thought that the cost of a really good nursing service in Scotland would be about £300,000. A great deal of work was done for public authorities at the present time, but little or no help was received from corporations. She did not think they could have a complete nursing service in Scotland by the voluntary system.

The Committee adjourned till the following week.

Correspondence.

LONG FREEDOM FROM RECURRENCE AFTER OPERATION FOR CANCER OF THE BREAST.

SIR,—I have read in your issue of January 24th (p. 156) with interest the notes by Mr. F. J. Steward of a case of recurrence of cancer of the breast after an interval of thirty-one years from the time of operation.

Some years ago I operated on a case of recurrence twenty-four years after operation. The right breast and the axillary glands were originally removed by Sir Thomas Smith for a typical scirrhous cancer, and I saw the patient with him after an interval of twenty-four years of good health. She had then recently developed a small lump in the skin, two inches below the middle of the right clavicle, and there was also a hard gland above the clavicle. I removed both of these, and the microscope showed typical breast cancer. A further examination while the patient was anaesthetized revealed hard tumours in each ovarian region, and she died within a year with growths in the cervical glands and visceral dissemination. The facts that the recurrence was in the skin of the chest and that it was typical breast cancer leaves no doubt that this was a case of genuine recurrence of the original tumour, and not of an independent new growth originating in breast tissue left behind at the first operation.—I am, etc.,

London, W., Jan. 6th. ANTHONY A. BOWLBY.

SIR,—Mr. F. J. Steward's case of recurrence of cancer of the breast thirty-one years after operation is somewhat analogous to a case published by me in your issue of June 24th, 1922 (p. 996). In the latter case the tongue had been excised for epithelioma in 1888. There was no recurrence near the site of operation and the patient lived a healthy hard-working life until 1922, when he died of cancer of the cardiac end of the stomach. This diagnosis was verified by *post-mortem* and microscopical examinations. I agree with Mr. Steward in his closing paragraph, where he states that the exciting cause in such cases must be general rather than local. They also go to show that a previous attack of cancer cured (in the ordinary sense of the term) by operation does not give immunity to another attack later on in life.—I am, etc.,

Warrington, Jan. 25th. J. S. MANSON.

TREATMENT OF PERFORATED GASTRIC AND DUODENAL ULCERS.

SIR,—I am heartily in agreement with your correspondents, Mr. Zachary Cope and Dr. Tonking, who do not consider that it should be generally accepted that gastro-enterostomy must be performed at the same time as the closure of the perforation in this type of abdominal catastrophe. On this question they are at variance with the recommendation of Mr. Percival Mills, whose experience favours the double operation. The reason for my opinion is not that I do not realize the excellent results of

immediate gastro-enterostomy, but because I believe that the routine treatment of a surgical emergency should be strictly in accordance with the emergency and not necessarily directed to the cure of the primary condition. Furthermore, surgical emergencies should be amenable to simple surgical remedies, and Dr. Tonking, I think, sounds a true note when he states that "for the general practitioner surgeon simple serc-serous suture and oversewing of the omentum is the safest course." It is the essential treatment of the perforation, together with dry sponging of the soiled peritoneal cavity. This treatment is on all fours with that applied to acute intestinal obstruction. Relief of the obstruction is then the immediate objective of the surgeon called upon to operate in a hurry.

Mr. Cope says that he used to be in favour of the combined operation, but that he now thinks that it is only occasionally advisable. This seems to me a subject on which a surgeon of experience may well have changed his mind. To such a one all things are lawful, but all things are not expedient. Expediency should be the governing principle in the surgery of acute abdominal diseases. The two main factors are the condition of the patient and the capacity of the surgeon. Time has a bearing on both: the hours that have slipped away before the perforated patient reaches the operating table, the minutes that will be consumed by the operation. For every hour over eight that elapses before the operation is done the mortality rises. For every minute that the operation is prolonged we ought to be sure that the patient gains some advantage. Anastomosis adds from twelve to twenty minutes to the length of the operation. Obviously, then, gastro-enterostomy is inadmissible in late perforations with widespread peritonitis, or where the condition of the patient is poor.

The other side of the picture, however, should be clearly viewed, and I must admit that here I differ from Mr. Cope, and I find more cases suitable for immediate gastro-enterostomy than I used to. My reasons are twofold: one is that cases treated by the double operation do undoubtedly have a very easy convalescence, but even more am I impressed by the number of cases of successfully sutured perforations which require surgical treatment at a later date. Gastro-enterostomy at the time of the operation for perforation is usually much easier of performance than anastomosis when the ulcer is closed, but the stomach or duodenum is surrounded by dense adhesions.

A long view of the treatment of perforated ulcer of the duodenum and stomach must include both the immediate and remote results. An opportunity occurred to me during a "major week" at St. Thomas's last year—when we had five perforations in the ward at the same time—of giving a simultaneous trial to the single and the combined operation. Three of them lived and two died. The age of the patient is one of the important factors, and the fatal cases occurred in patients whose ages were 59 and 63 respectively. The recoveries were all in younger patients. The age of the perforation is even more important, and in the fatal cases operation was not performed until twenty-four hours had elapsed, and in one of them it took place forty-eight hours after the perforation. The three surviving cases had perforated for five, six, and fifteen hours respectively. Two out of these three cases had immediate gastro-enterostomy and the ulcers were of the juxtapyloric type, where anastomosis yields such good results. I do not for one moment believe that the fatal cases would have been saved by the double operation. One died the next day, and the other from bronchopneumonia with a dilated heart eight days after the operation. Of the recoveries the quickest was actually in the case where only suture of the perforation was done, and he left hospital for a convalescent home ten days later; the other two left after seventeen and eighteen days respectively. Up to the present time, eight months from the date of operation, all three are in good health, but the future alone will show the benefit of the short-circuiting operation.

If we look at the remote results of simple suture of a perforation I think the old theory that such a complication cures the ulcer is completely exploded. It is about as true as the fiction that an appendix abscess destroys the appendix. It may be urged that the operation for the ulcer should be performed when the patient is convalescent from the first operation. It must be remembered, however, that we operate on human beings, and one of their characteristics is to dislike operations. Such has been my experience that I am accustomed to teach that at least 15 per cent. of former perforations require surgical treatment for persistent, or recurrent, dyspepsia. The combined operation, I believe, will do much to wipe out this undesirable figure. We may, however, see some patients back again with jejunal perforations, for there is a type of recurrent perforator difficult to eliminate altogether.

As I have stated at the beginning of this letter, which is perhaps too long already, I do not think gastro-enterostomy should be part of the *routine* operation for perforation. If circumstances of surgeon, anaesthetist, and patient are all favourable it is time well spent, but the superiority of the double operation over simple closure as an emergency operation can only be that the stomach gets rest. Immobility of the affected area—most often the region of the pylorus—for twelve hours is probably all that is necessary, and the chief value of the immediate anastomosis is that it reduces post-anaesthetic vomiting to a minimum, and indeed it often abolishes it altogether. An efficient gastro-enterostomy done for any particular lesion has this great merit, and in that way it will no doubt lessen the mortality of perforated gastric and duodenal ulcers from haematemesis or secondary perforation. These are, however, very rare causes of death after suture. Most fatal cases die from bronchopneumonia, peritonitis, or subphrenic abscess and its sequels. Rest to the stomach can be secured long enough to heal a sutured ulcer by good anaesthetization, and rectal saline feeding for the first twenty-four hours.—I am, etc.,

London, W., Jan. 19th. JOSEPH E. ADAMS.

SHAVING THE VULVA.

SIR,—At times we have as many fashions in medicine as ladies have in dress, and the *raison d'être* for some of the medical fashions, I fear, has as little substance as the fluffs and flimsies that go to make up feminine adornment. One of the medical fashions recently promulgated is to have the vulva shaved during labour, and we find obstetricians strenuously urging what they consider the absolute, imperative necessity for adopting this procedure. So far I am afraid I must own to being a heretic with regard to this new medical doctrine.

The first, but by no means the most serious, objection to this performance is that it must cause a distinct shock to the average woman's modesty. Here is the way I look at the matter. I am thoroughly convinced that every organ and every part of the body has its own special function. Luckily, in late years patient investigations have made known to us the part played in the human economy by many organs which previously were regarded as mere anatomical items. Take, for instance, the endocrine glands, which are now known to exercise a potent influence over our physical well-being. Only a few years ago the appendix, too, was regarded as a perfectly useless organism, a sort of anachronism. Now we know that it has important work to do, and does it. Before its function was discovered a friend of mine, an expert surgeon, always, on opening the abdomen, no matter for what purpose, laid hands on the appendix and, in spite of its being quite healthy and inoffensive, ruthlessly swept it away.

Pondering over these things I have been asking myself, Are the vulvar hairs mere purposeless appendages, or do they fulfil any useful function? And I am convinced that they perform at least two very important duties. They are these. During menstruation they convey away as far as possible from the body the menstrual fluid, which is meant to be a waste product, and they act similarly with regard to the lochial discharge, which is also a waste product. In this connexion it is worthy of note that the vulvar hairs are more or less non-existent till the time arrives for the performance of these two functions. They are not developed till puberty—the time at which the first menstrual flow occurs—and, of course, the girl child

cannot come into labour until after puberty. Now let us see what occurs with regard to the lochia when shaving has been done. The discharge, instead of being conveyed a distance from the vulva, remains on it and becomes caked, thus forming splendid pabulum for the hungry and migratory bacteria that are bound to be in the neighbourhood, and we all know that well fed bacteria increase and multiply with great rapidity. On the other hand, it may be urged that when shaving has not been done clotting might occur on the hairs. No doubt. But the tapering shape of the hairs, the fluidity of the lochial discharge as it emerges from the vulva, and the force of gravity all tend to prevent that clotting till the discharge has traversed a distinct distance from the vulva. The question arises, too, as to whether the bacteria can travel along the hairs, executing a sort of tight-rope dancing.

I am a thorough believer in Listerism, and accordingly would say, "Use suitable antiseptics certainly, but let the razor rest peacefully in its case." And instead of keeping the woman glued to the bed in the horizontal position for days and days, as was the fate of our mothers and grandmothers, allow her to sit up gently from time to time to promote drainage, which will be facilitated by the vulvar hairs. If that is done I feel confident that we will have gone a long way on the road towards preventing that terrible bugbear, puerperal sepsis, the opprobrium of the obstetric physician, and we will avoid the slight of being nicknamed the "barber-surgeons."—I am, etc.,

Cork, Jan. 10th. HENRY CORBY.

PUERPERAL INFECTION.

SIR,—It may be of interest to cite two of my recent cases.

The first, a primipara, was attended by me during her confinement in a nursing home where she got every possible attention. She had a rigor on the second day and died on the eleventh day of septicaemia. Forceps were used when the head was on the perineum. There was a slight tear which required one stitch. The labour was a long one, lasting some thirty-six hours.

My second case, a 2-para, I have just finished with. She was confined in her own home. She had a labour of some eight hours' duration. She also had a low forceps extraction, and also required a single stitch. There was, however, in her case a large congeries of inflamed veins in the right labium. This went on to abscess formation, during which the temperature was 102° F., pulse 120. The abscess ruptured into the vagina on the fourth day after confinement. The infection was *Staphylococcus aureus* and *Bacillus coli*. The temperature fell to normal on the abscess rupturing and the patient never looked back—this, although pus was pouring into the vagina and mixing with the lochia.

Now here are two cases. The technique was the same in both. How different the results! The nursing home case, in faultless surroundings, dies. The other, in a working-class home and flooded with pus, never turns a hair! I am of opinion there is some quality inherent in the patient which determines infection, puerperal or other. I have attended 2,550 confinements. My percentage of puerperal infection cases is 0.05 per cent.; these patients all died.

Will the specialists who take us general practioners to task kindly do two things? First, tell us their own percentage rate of puerperal sepsis and how they explain it. Secondly, will they engage in general midwifery work as we do for two years, practising all the asepsis they advise, and let us know again their percentage of sepsis and explain it. If they are not ready to do these things let them not criticize.—I am, etc.,

Glasgow, Jan. 15th. JAMES COOK.

ELECTRONIC REACTIONS OF ABRAMS.

SIR,—I notice that Sir Thomas Horder, when presenting the report of his committee of investigation of the "E.R.A." to certain Sections of the Royal Society of Medicine, referred to the "bigoted and *a priori* condemnation in which, actually, critics almost exclusively indulged, and of which alone the majority appear capable." I happen to be the only critic personally named in his address, and therefore feel bound to answer this accusation.

The charge of *a priori* condemnation is rather a superficial one, since it is notorious that the founder and followers of "E.R.A." have taken every precaution to prevent impartial examination of their machines or methods. The machines are leased at a fantastic price, and under a bond which forbids them being opened. The alternative to buying a machine is to co-operate with electronic practitioners. This, however, is a matter of extraordinary difficulty. In this respect Sir Thomas Horder's committee evidently has had the same experience as all previous investigators, since it appears that it was unable to arrange to test the diagnostic power of "E.R.A." practitioners, subject to the usual condition that the results should be published whether positive or negative.

As regards the charge of bigotry in criticism: I, like many others, have attacked the "E.R.A." practitioners on ethical and scientific grounds. The ethical criticism is that they are exploiting a new form of diagnosis, the value of which has never been established by any form of impartial test, and that they consistently evade any such test. This charge Sir Thomas Horder endorses and emphasizes. He even goes further, and suggests that the electronists themselves know that their methods will not stand impartial testing, but that nevertheless they are perfectly willing to use these methods in practice. From the scientific point of view I consider that the claims of the electronists appear absurd, and I agreed with the opinion of the *Scientific American* that there was no evidence to show that the electronic reactions even exist. Sir Thomas Horder definitely endorses this opinion as regards the regular followers of Abrams (conclusion 2 in his report). The very interesting positive results recorded by his committee were obtained with Dr. Boyd's "emanometer," and we are told that this instrument is not a minor variation of the "E.R.A." machine, but is "a design *de novo* based on a different conception of the phenomena involved," and that the conclusions arrived at by the committee "leave the position of the practising electronist as scientifically unsound and as ethically unjustified as it was before."

I cannot see that Sir Thomas Horder has justified his sweeping charge of unfair criticism against previous critics of "E.R.A." Past criticism of "E.R.A." may have been founded on scanty evidence, but this is bound to be the case whenever a new remedy is exploited and its methods are kept secret. Indeed, the charge of "bigoted and *a priori* criticism" is one of the stock lines of defence of every quack medicine vendor. I consider that it is unfortunate that this particular mode of defence of secret remedies should have received any kind of endorsement from high medical authority.—I am, etc.,

University College, London, Jan. 24th. A. J. CLARK.

SIR,—The members of the Society of Electronic Medicine in London desire to submit to your notice the following commentary on Sir Thomas Horder's recent address on the electronic reactions of Abrams delivered before the Royal Society of Medicine.

It should be stated by way of explanation that this commentary, which is somewhat critical, is not to be understood as attempting to detract in any way from the value of his communication or as throwing any doubt on the sincerity of the scientific spirit prompting him to make it.

The value referred to lies, of course, in the fact that Dr. Boyd of Glasgow has succeeded in satisfying Sir Thomas Horder and his co-observers that an electronic reaction exists, for which achievement the former deserves no little praise, both on account of his scientific effort and infinite patience; but more than this bare fact the interested committee is apparently not prepared to regard as demonstrated.

The statements, therefore, to which we are bound to take exception in Sir Thomas Horder's address concern *only* what by his own admission he knows as yet nothing about—namely, the practical value of Abrams's ideas in the field of medicine; and we note with regret that these statements are made with no less assurance than he displays in connexion with the experimental work of which he is certain.

We make no comment upon certain pronouncements, which, although they are to be found in the text of what has been termed "the full report," were not embodied in the actual address itself, but confine our remarks solely to

the latter as reported in the columns of the BRITISH MEDICAL JOURNAL of January 24th, 1925.

There a statement is published of such a nature that we, as a society, cannot but request Sir Thomas Horder either to assure us that owing to misinformation he has been in error or to produce satisfactory evidence supporting it.

This statement is to the effect that to employ Abrams's treatment is "scientifically unsound and ethically unjustified." The only reason he gives for making it is the fact that the scientific basis of the treatment remains uncertain; but while this is undeniably so, we must point out that no such objection can validly be made to the employment of a treatment which has been proved to give good results. If Sir Thomas Horder were to be taken at his word in this matter of ethics, then *ipso facto* a host of useful therapeutic measures would necessarily stand condemned in his eyes, including no doubt several hitherto employed by himself. Indeed, we should have to conjure up the curious vision of this busy physician "only experimenting privately" with methods the scientific rationale of which is still obscure to him—surely a *reductio ad absurdum!* We therefore are curious to know what real justification, if any, he can plead for taking up so extraordinary and autocratic an attitude.

In a communication to the BRITISH MEDICAL JOURNAL of January 10th (prior to Sir Thomas Horder's address) a member of our society took the trouble, in view of widespread misunderstanding, to dispose of all possible grounds of accusation on the score of unethical conduct. Facts were stated showing that the society had not made exaggerated claims on behalf of the Abrams system, and that they had taken all possible steps towards removing the mystery associated with it by a certain body of individuals to whom, and to whom only (also on account of their exaggerated claims), the derogatory term "Abrams cult" could be applied. To say that no definition of a "genuine Abrams worker" was afforded by that communication is so transparently inaccurate that anyone interested can simply be referred to it to discover the truth for himself.

It must be admitted that Abrams himself was not, according to our ethical ideas, free from the blame attached to the cult which sprang up as a result of his conceptions, but in our view his extraordinary temperament (common enough in geniuses) and the opposition he had to face ought to be taken into consideration when we condemn him for his mistakes. All who knew him intimately believed him to be honest, and at any rate he seems to deserve greater credit for his ideas than Sir Thomas Horder is inclined to give him.

In a letter to the *Lancet*, February 16th, 1924, the same member of our society said, "However interesting a general correspondence between 'E.R.A.' and clinical findings may be, it would be surprising to discover that they absolutely fitted in with one another." This letter, which in every way reflected the cautious view of Abrams's claims taken all along by our society, also stated, "The first reply to the criticisms of 'E.R.A.' lately begun in the British medical press contains an offer to have the results of treatment along one important line publicly investigated; and the spirit of this offer has but one significance—namely, that Abramsites desire only to maintain a faith based on reality." This offer at the time was not deemed worthy of acceptance.

If to these observations we add one to the effect that medical journals have experienced difficulty in finding room for our communications, little significance can be attached to Sir Thomas Horder's complaint, "neither coaxing nor baiting nor even sincere invitation has induced practising electronists in this country to tell us exactly what they are doing."

Statements we have made in the past (some nearly a year ago) indicating a moderate attitude cannot be construed as "belated misgivings," nor are we prepared to admit the parallel of "turning King's evidence" either in respect of "turning" anything at all or in respect of identifying Sir Thomas Horder's authority with the high authority implied by this well known phrase.

As for the idea of no "electronist" being public-spirited enough to acknowledge the "glaring inconsistencies" in his work, we suggest a very cogent reason why publication of certain tests was deemed "inadvisable pending further research." It is that our researches (apart from treatment results) have not yet enabled us to say more than that an as yet undefined relationship exists between "reactions" and disease. We cannot at present do more than use these reactions as general indications guiding treatment in conjunction with other ordinary and "orthodox" indications. We must point out that *frequent* examination of a patient's blood reduces the chances of our being misled through the interference of factors at present beyond our control, and if we check our findings by clinical examinations it cannot be said that we are likely to make more mistakes than the average physician. "Tests," on the other hand, involve one or few examinations of isolated sets of specimens without the help of clinical observation.

In spite, however, of these considerations, from which it may be taken that our accessory diagnostic procedures are far from being mathematically accurate, remarkable diagnostic accuracy under "test conditions" has occasionally been achieved. Special reference may be made to the test to which Dr. Mather Thomson submitted himself, devised by Lieut.-Colonel Tizzard, Major Lefroy, and Dr. C. B. Heald. This test, which resulted in correctly diagnosing certainly three, and possibly four, unknown bloods out of four, was not fully quoted (if even referred to) by Sir Thomas Horder. We hope that Dr. Boyd's inventions may lead to the discovery of a technique enabling this kind of result to be attained more consistently.

Such successes, even if rare, certainly lend weight to our very general claim in regard to the practical value of Abrams reactions. History has shown that men do not wait for phenomena to be accurately understood and controlled before they begin to make what use of them they can.

It should be added that our investigations into the explanatory principles underlying "reactions" and treatment effects have not yet borne fruit, and we do not believe that correlation of certain reactions with specific disease processes has yet been satisfactorily worked out. No statement implying the contrary has had the approval of our society or of its president. All this—which evidences a sincere and moderate claim—was embodied in a communication sent to the BRITISH MEDICAL JOURNAL prior to Sir Thomas Horder's address, for which, however, no space was then available. Yet we are said to be "masonic" in our methods!

The chief claim which we, as a body of investigators, make is that excellent results have occurred from the treatment of patients by Abrams's oscilloclast, often in cases uninfluenced by all "orthodox" methods (including the most modern psychic treatment) and generally in organic conditions. The society assumes no responsibility whatever in connexion with a new machine, not yet tested, which has been placed on the market by one of its members (Dr. Laughton Scott) or with his particular theories; but reference may be made to the fact that the cases recorded in his recently published book were treated by Abrams's oscilloclast.

To sum up, our society can see no justification for several statements made by Sir Thomas Horder which have no real bearing on the kernel of his recent communication, nor is it in any way impressed by the appearance of authority with which he attempts to invest his committee, as evidenced by such statements as that their conclusions "give no sanction" for the employment of certain measures in practice by other qualified physicians. We see nothing to justify the view that we have been "manœuvring for position" any more than Sir Thomas Horder appears to have been doing, and we can assure him that the imminence or otherwise of his address was not a matter the influence of which was in the least apparent amongst us.

His committee is reminded that only in so far as they have been observers of another man's work are they qualified to express guarded opinions on the subject of Abrams's reactions, while on the subject of any diagnostic value they may have or of investigation into the results of treatment they appear to have no justification for expressing any opinion at all.—I am, etc.,

OSCAR PARKES,
Secretary, Society of Electronic Medicine.

London, S.W., Jan. 26th.

CANCER OF THE OESOPHAGUS.

SIR,—In the discussion on Mr. Souttar's paper on cancer of the oesophagus, in the Surgical Section of the Royal Society of Medicine (reported in abstract in the BRITISH MEDICAL JOURNAL of January 17th, p. 115), Sir StClair Thomson raised the important question of the place of gastrostomy in the treatment of this condition; and asked, in view of Mr. Souttar having made only the slightest reference to this mode of treatment, whether gastrostomy was ever required for gullet cancer. Sir Charters Symonds expressed the opinion that the question of gastrostomy did not arise at the present day at the hands of those who were expert at modern intubation methods, as it was possible to deal with any region of the gullet by tubage.

In spite of this authoritative pronouncement by the experienced originator of the first really satisfactory form of intubation apparatus for gullet stricture—the epoch-making Symonds funnel—I am emboldened to express the opinion, based also on large experience, that Sir Charters's pronouncement excluding gastrostomy is too absolute. I admit that there must be few cases, if any, of cancerous stricture of the middle three-fifths of the gullet which cannot be safely and successfully per-endoscopically bougied up and intubated by experienced and skilled manipulators; but I venture to affirm that there are some very tight and tortuous strictures in the supraclavicular gullet above and in the phreno-cardiac portion below where difficulties may be encountered, and where it is safer to resort to gastrostomy, and where there is a very real danger of perforation even in the most expert hands. The same observation also applies to some tight pharyngo-oesophageal and oesophago-gastric strictures. I agree with Mr. Souttar and Sir Charters Symonds that intubation, when it can be carried out, and with safety, is always more comfortable and satisfactory to the patient than gastrostomy, and has the advantage over it that the patient is able to swallow the saliva which is usually enormously increased in gullet cancer.

Radium therapy has a much more restricted field of application than intubation, but in properly selected cases, and when it acts favourably, it has the advantage over intubation or any other form of treatment that it may bring about either temporary arrest of the growth, or marked reduction of the growth, or even apparent gross disappearance for a considerable period, the latter amounting in exceptional cases to several years. Such gratifying results were reported recently by Guisez in his Semon Lecture, and also by me in a paper published in the JOURNAL of December 27th, 1924 (p. 1196).

In conclusion, whilst thinking that Sir Charters Symonds, Mr. Souttar, and others rely too much on intubation, to the exclusion of other methods of treatment (in exceptional cases), such as radiotherapy and gastrostomy, at the same time I think that Mr. Souttar's substitution of spiral wire funnels for gum elastic ones promises to be a distinct advance.—I am, etc.,

London, W.1, Jan. 23rd. WILLIAM HILL.

A NEW OUTLOOK ON CANCER.

SIR,—Dr. James Young's paper in the BRITISH MEDICAL JOURNAL of January 10th (p. 60) is a stimulating statement of facts and of reasoning based thereon. The facts have wide biological and pathological bearings, a few of which I beg leave to indicate.

So long as I left the label "sporozoa" on the alien cell inclusions and alien free bodies of cancer, molluscum, syphilis, etc., I paid but little heed to the finding (whether by others or myself) of bacteria or other fungal forms in cultures made from cancers. This attitude was reversed when I realized that these alien bodies are fungal and are akin to the genus *Synchytrium*.[1]

The group Phycomycetes is now taken to include synchytrians and other Chytridinae, with Mucorineae and other orders of fungi. They have affinities with the Mycetozoa. The stalked sporanges of the common mucors are familiar; as is also the fact that when mucors are cultivated anaerobically in presence of glucose they grow in their gemma-form—that is, like yeast cells—and, like the latter, they cause alcoholic fermentation.

[1] *Lancet*, vol. ii, 1921, p. 495.

From a study of Dr. Young's illustrated paper[2] I concluded that the forms he has described are cultural modifications of the same fungi that constitute the alien cell inclusions, etc., of cancer. In some of Dr. Young's microphotographs plasm (?=plasson) is seen to have grown out from the nuclei of cancer cells; I have little doubt that such growth started in pseudo-karyosomes, such as are described in *Protozoa and Disease*, Part ii, Chapter xii.

In water cultures of molluscum corpuscles streaming of amorphous protoplasm is commonly the first visible sign of active life (see *Protists and Disease*, Chapter vi). A similar commotion was observed by Löhnis and Smith in the plasm stage of some of their cultures of *Azotobacter*.

Synchytrium taraxaci is the foundation species of its genus. Synchytrians are allied to coccidia and only their coccidium-like stages are described in textbooks, but I have found also a dense chromidial (amorphous) stage.[3] In *S. endobioticum*, the cause of wart disease (Kartoffelkrebs), E. Köhler has described a coccus-like form, which he regards as degenerating; but, in *S. taraxaci*, I have found, and described in a paper as yet unpublished, a quite similar form as the initial stage of the amorphous chromidial modification just mentioned.

The association of a filterable virus with specific cell inclusions is frequent and noteworthy: we should remember that the first demonstration of a filterable virus was in mosaic disease of the tobacco plant (Ivanovski, 1892), also that in this same plant disease cell inclusions occur, which phytopathologists agree in regarding as being of the same nature as Guarnieri's and Negri's bodies.—I am, etc.,

London, Jan. 10th. J. JACKSON CLARKE.

HEART STRAIN.

SIR,—The following statement in Dr. Brockbank's very interesting lecture on heart strain (BRITISH MEDICAL JOURNAL, January 10th, p. 57) seems to me to be open to considerable criticism.

> "The second reason is that the pressure in the aorta, whatever it is, which drives blood through the incompetent valves, acts at its full strength on every part of the interior of the left ventricle of the same area as that of the leakage in the valve, and is not diffused all over the wall of the ventricle. In other words, if there is a force of 100 mm. of mercury acting on a valve leak of 1/4 in. square, there will be a force of 100 mm. acting on every square 1/4 in. of the ventricle, and not 100 mm. diffused all over the ventricle."

Theoretically, of course, this is quite true. Does it then follow that a valve leak of 1/4 in. is more dangerous than one of double the size? Would one of 1/10 in. be more dangerous still? In other words, is the danger in inverse proportion to the area of the opening?

The principle underlying Dr. Brockbank's statement is the old familiar one of the Bramah press, whereby the pressure exerted by a piston working in a tube of small cross-section is conveyed through the liquid in a rigid container to a piston working in a tube of much larger cross-section, and is thereby greatly increased. The considerable distance through which the smaller piston moves, in one or repeated strokes, is, of course, the means by which mechanical advantage is gained in magnifying the pressure.

But three things are here to be noted: (1) There must be no valve opening out of the container; (2) the container must be approximately rigid; (3) time must be given for the liquid to flow through the smaller opening.

None of these data hold good in the case of the heart. (1) There is what is equivalent to another valve opening from the ventricle—namely, the remaining apposed edges of the semilunar valve. These (unless adherent) are only held together by exactly the same pressure as is exerted from the ventricle. In other words, in no case can the intraventricular pressure caused by a partial valve leak be greater than that caused by a complete valve leak. Practically, it must be considerably less, owing to the slower rate of flow. (2) The ventricular walls are, of course, far from being rigid; that is to say, the "give" at any one part will take off the pressure, and so prevent the full theoretical pressure being exerted until the tension is very high. (3) The rate of flow, being much slower through a smaller

[2] *Edin. Med. Journ.*, 1924, p. 163.
[3] *Lancet*, February 16th, 1924, p. 361.

opening, renders it impossible for a dangerous tension to be exerted during the small period available in diastole.

From these considerations I think it is apparent that theoretical estimations have little place in reckoning up the danger of an incompetent aortic valve. The utmost danger would be found if the whole valve were deficient—that is, if the opening were at its maximum width.—I am, etc.,

Grangemouth, Jan. 17th. THOMAS PULLAR.

FREUDIAN DOCTRINE.

SIR,—Criticism of the body of doctrine known as psycho-analysis is undoubtedly needed. But if such criticism is to be of any scientific value it should be both informed and constructive. Informed criticism can come only from those who have become practically acquainted with the method by carrying out a number of prolonged analyses on patients—and, still better, who have also submitted themselves to prolonged analysis under the direction of an experienced analyst. Constructive criticism can come only from those who have accumulated a wealth of clinical data by this same method of "deep" analysis, only to be acquired by many years of experience.

The number of psycho-therapists in England outside the ranks of the avowed Freudians (and also perhaps inside) who have fulfilled these conditions is exceedingly small. This is the difficulty which such a society as the Medical Section of the British Psychological Society has to face, in deciding upon such a general discussion as that suggested by your correspondents.

The Medical Section is dominated, not by Freudians, but by a firm determination to probe the psychological secrets of mental disease to their depths, to base theory upon empirical evidence and not upon *a priori* arguments, and to be undeterred by mere *ex cathedra* statements, from whatever quarter. Discussion of Freudian doctrine among those adequately qualified to appreciate both the problem and the facts will always be welcomed in our society. Any other kind of controversy would be rather reminiscent of the fight between the elephant and the whale.

I should perhaps add that I am not a Freudian.—I am, etc.,

WILLIAM BROWN,
President of the Medical Section, British Psychological Society.

London, W.1, Jan. 24th.

SIR,—In my previous letter (January 10th, p. 93) I said, on behalf of my colleagues, that we were "prepared at any time . . . to participate in any suitable conference, symposium, or discussion on psycho-analysis arranged by any medical or other scientific society." Sir Bryan Donkin, in his letter published in your issue of January 17th, replies that "the kind of discussion proposed by Dr. Ernest Jones . . . is very different from that which I suggested as desirable." The only alternative remaining would therefore appear to be that all those who have made a proper study of the subject, and have had personal experience of it—that is, psycho-analysis—should be excluded. Such a course might succeed in the aim of securing more complete unanimity, but it would surely be a very novel form of "scientific discussion."

I did not give it *as my opinion* that psycho-analysis had been frequently and extensively discussed "among well accredited medical and psychological authorities" (including, of course, both opponents and supporters of the method). I stated it as an easily ascertainable fact, and I am astonished that Sir Bryan Donkin should again assert the contrary in place of verifying the statement. He likewise refuses to accept my correction of his assertion that there was no discussion of the paper I read before the Royal Anthropological Institute, but merely multiplies his original misstatements by now saying: "This, however, took place on the occasion of an address by the President of the Institute and a following paper read by Dr. Jones, on the relation of anthropology and psycho-analysis, of which no criticism was made and to which no subsequent replies were admitted." The facts, which are easy to verify, are, on the contrary, that (1) my paper was *not* read on the occasion of the presidential address, but was the only one read at that meeting; (2) there *was* extensive criticism made of it in the discussion that followed at the time; and (3) subsequent replies *were* admitted, for a sharp criticism of it was published in the following number of the official organ of the Institute.

In his previous letter, Sir Bryan Donkin said that neither of two particular attacks on psycho-analysis "has been replied to by any prominent advocate of this doctrine." Though I cited extensive replies that have been made twice over to them by prominent advocates of this doctrine, in just the place one would expect to find them, Sir Bryan Donkin again writes: "(I) can repeat confidently my statement concerning the non-existence of any adequate criticism by leading psycho-analysts of the two books." His first statement has been shown to be as misinformed as his other pronouncements in connexion with psycho-analysis, and I doubt if inserting the word "adequate" this time will help him much, for only those who have read the replies in question can judge of their adequacy.—I am, etc.,

London, W.1, Jan. 20th. ERNEST JONES.

SIR,—I am one of those who would welcome a dispassionate discussion on the foundation principles of modern medical psychology. Psychologists are the very people who should be experts, not only in the theory of "emotion," but in the control, not repression, of their own emotions, and yet the subject suggested for discussion seems to excite anything but dispassionate treatment.

Obviously we are not going to make much progress if Freud is to be regarded as a medical Guy Fawkes, and we are to be invited to follow Drs. Wohlgemuth and McBride in helping to light the bonfire in which his effigy is to be consumed. Science is not to be advanced this way.

Perhaps the Royal Society of Medicine could promote a discussion on broad lines on this great subject. I would suggest that a preliminary private meeting of representative medical psychologists of different "schools" should be called together to devise the programme, and, without stifling discussion, it ought not to be beyond the wit of man to bring together those who, with the honour of the medical profession in view, would sanely and honestly state their views.—I am, etc.,

London, E.C.2, Jan. 16th. CHARLES F. HARFORD.

WOMEN DOCTORS.

SIR,—It seems to me that your correspondents (January 24th, p. 192) present a very distorted view of the situation.

Taking Miss Ramsay's figures, not more than one woman to every twelve men has any reason to expect a resident hospital appointment on qualifying, and then only if she is as capable, or more so, than any one of the twelve men with whom she happens to be competing. Therein, I think, lies the answer to the question about the appointment of women doctors in hospitals, rather than "pure prejudice on the part of the male colleague." The lack of a hospital appointment is no reason why she should not go in for general or dispensary practice at home or abroad. Many of her male colleagues do so.

The public does need medical women in many spheres. It is up to such as these "45 out of 78 who have already held public appointments" to cast around and find where they are wanted, instead of seeking for a series of temporary posts.

To quote Dr. J. Walter Carr, C.B.E., for one instance, ". . . in India there are mission hospitals actually closed because no women doctors can be found to staff them." We need more of the stuff that the medical women of the generation just passing were made of—women willing to continue to carry out the responsibilities undertaken by the pioneers.—I am, etc.,

Birmingham, Jan. 26th. FRANCES BRAID.

SIR,—Dr. Robinson's figures (January 17th, p. 140)—78 women applying for a resident medical officer's (hospital) appointment with £100 a year—are nothing compared with those given by a client of mine who, after repeated failures to secure any such hospital appointment, has

recently accepted an indoor assistantship in the Midlands, also with only £100 a year. She wrote me that she was selected out of 180 candidates! As to the permanent outlook for medical women Dr. Ramsay may be quite right. That there is at all events a temporary glut such startling figures conclusively prove.—I am, etc.,

Torquay, Jan. 24th. HENRY BAZETT.

TESTS FOR DRUNKENNESS.

SIR,—We have read with interest and amusement the address of Sir James Purves-Stewart delivered on January 13th before the Society for the Study of Inebriety. At the same time we must confess to a certain feeling of disappointment; for we had hoped that the harassed divisional surgeon might perhaps derive from this lecture valuable assistance in the most thankless and difficult of all his tasks—something a little more helpful, for instance, than a half-hearted instigation to the performance of lumbar or cistern puncture! And there is one point on which we must at once join issue with Sir James Purves-Stewart—namely, his suggestion that "the police surgeon may be too open to suggestion." We venture to submit that the police surgeon is very much less open to the charge of bias than is the medical man called in by the suspect for the express purpose of proving the latter's sobriety. With a long experience of many police divisional surgeons we have no hesitation in saying that we have implicit confidence in their absolute impartiality. The divisional surgeon has nothing to gain by supporting the provisional diagnosis of the arresting police officer; in point of fact he very frequently reports on his man as being sober, and we are satisfied that when the divisional surgeon has any doubt as to the prisoner's sobriety the prisoner gets the benefit of the doubt.

We also contend that a police surgeon attached to any of the big stations in Central London has an unrivalled experience in the recognition of inebriety in all its stages, and for ourselves we would prefer to rely on the opinion of such an experienced and impartial observer rather than on that of any other medical man, however eminent in his own particular line.

Again, we do not feel that Sir James Purves-Stewart has sufficiently emphasized the notorious fact that drunkenness may be a very transient phenomenon. We can most of us recall incidents in the course of bump suppers, or similar festivities, illustrative of this point—for example, the inco-ordination and dysarthria of the undergraduate at 10.30 p.m. who has pulled himself together and is conversing amicably (though perhaps with a slightly overdone solemnity) with the Dean at 11.15 p.m. The same sort of thing occurs not infrequently at the police station, if we substitute a distinguished consultant for the Dean of the bump supper days.—We are, etc.,

CHARLES A. BALLANCE.
London, Jan. 27th. MAURICE A. CASSIDY.

THE EFFECT OF LEAD ON RED BLOOD CELLS.

SIR,—Your note (January 17th, p. 131) on this subject cannot be passed without comment, because haematology has been so long neglected in England that it may be taken too seriously.

Experienced practitioners appreciate the fact that mild and early lead poisoning is often difficult to diagnose or to exclude. I had been diagnosing cases of plumbism in a village in County Durham for eighteen months before any medical man would accept the diagnosis. The worst case was an anaemic young woman, who had complete paralysis of arms and shoulder girdles, had to be spoon-fed by others, and was just able to totter across the kitchen of a small cottage. Two consultants in Newcastle diagnosed an unusual case of paralysis, gave a hopeless prognosis, and, as she was engaged, positively banned marriage. The man insisted on marrying the girl, even if it was only to nurse her till she died. Two years after I attended at her first confinement, when she was well, except that her hands were weak. At last I found a case with a blue line on the gums which nobody could deny, and an investigation followed. The engineers of the Newcastle waterworks found that, in spite of their directions, houses had been erected here and there with a very excessive length of lead piping, and that my cases and suspected cases were entirely confined to these houses. The chief engineer was particularly interested in the woman because her home had the greatest length of lead piping in the village, and he pointed out to me how marriage removed her from this to her husband's home, where the main pipe passed close to the door.

These cases happened many years ago when it was difficult to find evidence sufficient to warrant the diagnosis in such cases. About twenty years ago it began to be recognized that Grawitz and Hamel had proved the value of blood examination as an aid to diagnosis. In 1907, by treating various minor ailments with small doses of lead acetate, I satisfied myself by repeated examination of films that this was a most important advance. Then came Sir T. Oliver's book on lead poisoning (1914), in which he declared that in the North of England blood examination had proved of no value. As my work was done in the North of England I may state that he and his experts were hopelessly wrong.

In commenting on the blood in plumbism, Dr. Cecil Price-Jones says, "basophilic stippling is sometimes observed, but not so frequently as was sometimes supposed." In my last fourteen cases it was not once absent. Besides, it is futile to single out punctate basophilia. Ignoring punctation altogether, those who persistently wash it out should look for polychromasia, megalocytosis, Jolly bodies, Cabot rings, and erythroblasts; these are less easily banished from a film, and accompanied basophilia in my fourteen cases. Even those who find basophilia in the majority of healthy persons might find a safeguard in so doing. It would probably puzzle them to be told that for years I looked out for leucoblasts in these films and found them last year for the first time in a case of lead poisoning.

I had the privilege of stating my experience in your pages[1] and explaining my technique,[2] and would conclude with these words: In plumbism, long before muscle, nerve, or intestinal symptoms, the metal acts on the marrow, and the blood promptly shows definite, constant, and conspicuous changes. Consequently there is no reason why industrial absorption should ever advance to lead poisoning.—I am, etc.,

Ealing, W.13, Jan. 18th. ROBERT CRAIK, M.D.

ACIDOSIS AND ACETONAEMIA IN RELATION TO SEA-SICKNESS.

SIR,—The interesting contribution by Surgeon Sydney Jones, of R.M.S. *Aquitania*, in your issue of January 17th under the above heading encourages me to submit some observations I have made on this subject during the past ten years. The relation of acetonuria to sea-sickness and the allied conditions—air-sickness, train- and car-sickness—was first brought to my notice in 1915, when, as a naval surgeon attached to the airship subsection of the (then) R.N.A.S., I was called upon to examine officers and ratings from the special point of view of fitness for flying. Acidosis was regarded as of considerable importance; it was always suspected and the urine tested for acetone if the applicant was unable to hold his breath comfortably for more than thirty seconds. This simple test proved to be almost infallible. Should the officer or rating have had previous experience in flying a history of air-sickness was present in all cases of positive acidosis. By "air-sickness" a condition identical with sea-sickness is inferred, due to the motion of the airship, seaplane, or kite balloon, and not in any way due to high altitudes, this point being clearly stressed by the fact that air-sickness was always worse in kite balloons, working at a lower altitude, than in any other form of aircraft. During two years with the R.N.A.S. the close correlation between air-sickness and acidosis appeared to me to be an established fact. Part of 1917 and 1918 spent in the Eastern Mediterranean in surface craft did not give many opportunities for pursuing this investigation, but a number of outstanding cases of

[1] BRITISH MEDICAL JOURNAL, 1923, i, 103. [2] Ibid., 1924, ii, 788.

intractable sea-sickness, described by the men themselves as "absolutely paralytic," bore out Surgeon Sydney Jones's recent observation that 99 per cent. of cases of vomiting at sea, not due to an obvious organic lesion, are accompanied by increased acidity and acetonuria. Since resuming civil practice after the war, acetonuria in children has claimed my attention, and some of the following observations bearing on the subject were published by me in *St. Bartholomew's Hospital Journal*, March, 1924, under the title "Some practical points in the diagnosis and treatment of acetonuria in children."

A simple bedside test for acetone is of first importance, and can easily be performed by using a small endolytic (capillary) tube containing sodium nitro-prusside and ammonium chloride (put up by Fletcher and Fletcher, of Holloway, N.). One large drop of urine is taken on a sheet of notepaper, and saturated with a common washing soda crystal the size of a pea. This saturated urine is allowed to run up the tube by capillary attraction, the reagent having been shaken to the lower end; if acetone is present in the urine in quantity a petunia colour develops in about thirty seconds, and even when only small amounts are present a rosy or amethyst flush is noticeable in about a minute or two. When acetone is absent the powder dissolves with nothing more than a pale straw coloration, which does not develop further. A routine examination for acetone in the urine of all children coming under notice reveals the fact that it is found in more cases than one would suspect. All cases of recurrent bilious attacks, tonsillitis, pneumonia, and practically all febrile attacks only account for about 50 per cent.; in the remaining 50 per cent. acetone has been detected as a result of routine examination where it would not have been otherwise suspected. These children came under observation because they were out of sorts, had bad appetites, were bad-tempered, or did not gain weight; several recent cases evidenced disordered action of the heart and nothing else.

In treatment, unlike Surgeon Sydney Jones, I hold a brief for glucose. Cases of cyclic vomiting do not appear to tolerate alkalis by the mouth during the attack, but take glucose readily, and retain it when everything else is returned. A heaped-up teaspoonful of glucose in half a teacup of cold water, repeated every two hours until four or five doses have been taken, has produced in cases of urgent acetonaemia in children 5 or 6 years old the same wonderful improvement described by Surgeon Jones as resulting after the treatment of his distressing cases of sea-sickness with high rectal injections of sodium citrate. Feeding with glucose appears to me to give relief in practically all cases, and if no symptoms are present it has the effect of clearing up the acetone in the urine. Concerning Surgeon Jones's final remark on the advisability of correcting hyperacidity before embarking, I have recently carried out very successful prophylactic treatment on these lines among juvenile patients who suffer from train- and car-sickness associated with acetonuria, on returning to boarding school after the holidays. A round of parties and dances, with concomitant indiscretions in diet, had produced in these cases a lively acetonuria, but this was controlled by one day of meagre diet and several feeds of glucose at the end of the holidays, the long train or car runs being subsequently undertaken without any inconvenience. The same prophylactic measures do not appear to be so necessary on returning home at the end of term, as the healthy and simple diet of the average preparatory and public school does not seem to be very productive of acetonuria.—I am, etc.,

MARMADUKE FAWKES, M.B.,
B.S.Lond.

Midhurst, Sussex, Jan. 21st.

ASSETS OF THE I.M.S.

SIR,—The Indian Medical Service, "for official reasons," is moribund. Hence it is worth while ascertaining what are its assets. The formation of the Clive Fund in 1770, in which both medical and combatant officers had rights, was followed at successive periods by the institution by the Medical Service of separate funds in the respective Presidencies. In 1807 the Madras Medical Fund was inaugurated on the basis of an assurance society; by 1829 subscriptions by official orders were compulsory. The ends to be met were the provision of pensions for widows and children, passages for officers on sick leave, and of annuities for senior officers on retirement. The latter object was not only of value to individuals but to the whole service, in that an inducement was offered for early vacation of appointments by seniors. Lieut.-Colonel D. G. Crawford, I.M.S., in his work *A History of the Indian Medical Service, 1600-1913* (vol. i, p. 482) thus describes the fate of this fund:

"The assets, liabilities, and management of this Fund were transferred to Government from 1st April, 1870, by Despatch No. 101 M. of 9th December, 1869; and after that date no new members were enrolled. In 1880 it was decided that it was not desirable to offer to members a refund of their subscriptions. For many years annuities from this Fund continued to be granted yearly. All members have long since retired, but some are yet living who still draw their annuities."

It will be noted that transfer to the Government of India of assets and liabilities of the fund was effected by official orders. This method usually leaves but a Hobson's choice to those addressed.

Having regard to the existence of organized military and civil departments dealing with finance, it is unlikely the extra strain caused by the transfer of funds could have rendered necessary other than trivial addition to their personnel and consequent cost of management. Tradition holds that the capital transferred to Government in 1870 was a very substantial sum, whilst, as the above extract shows, further increments of liabilities were prevented by the Government directing that no new members be enrolled. In 1880 it decided that it was "not desirable" (!) to refund subscriptions to former members, thus affording an unexplained departure to its mode of dealing with the Bengal Medical Fund, where repayment was made to subscribers *minus* 10 p.c. (*sic*).

Obviously, at the present date, demands upon the fund must have vastly decreased, whilst interest on capital should have accumulated. Unless, therefore, documents dealing with the transfer gave the Government of India, *with the consent of the subscribers*, absolute right to the capital coupled with the power exercised by it to refuse further liabilities by enrolment of new members, it follows that it is in possession of funds as trustee beyond the requirements of liabilities. It seems to me, therefore, well within the functions of the British Medical Association to ascertain from the Secretary of State for India whether this view of the matter can be maintained, or whether the transfer was ordered on philanthropic principles. Should the latter suggestion be untenable, there are doubtless immediate heirs of subscribers who would not object to a share of the fund capital, whilst there are purposes, scientific and charitable, which might be benefited by gifts *in memoriam* of the I.M.S.—I am, etc.,

W. G. KING,
Colonel I.M.S.(ret.).

Hendon, N.W.4, Jan. 23rd.

WHAT IS THE DEFINITION OF "ATTRIBUTABLE" IN WAR INJURIES?

SIR,—I desire to obtain opinions upon the following case. A soldier in September, 1918, when on duty, was bitten in the neck by a horse. The wound healed with a "lump" in the scar and impaired movement of neck and arm. He was demobilized in March, 1919, with a scar and lump; he worked intermittently, the lump getting larger, but did not consult a medical man until February, 1923, when the tumour involved too much for operation. Radium tubes did no good, and the man died, after repeated haemorrhages, in 1924.

The local Pensions Board turned the widow down on the ground that the cause of death was not attributable to the wound; and on appeal, though the chairman admitted the sequence of the disease and wound, it was ruled that too much time had elapsed from the receipt of the injury to the death, and so it could not be attributable.

I therefore should be glad of opinion on the subject.—I am, etc.,

P. BROOME GILES,
Colonel A.M.S.(Ret.).

Folkestone, Jan. 18th.

Obituary.

SIR JAMES MACKENZIE, M.D., F.R.C.P., F.R.S.,

Director of the Institute for Clinical Research, St. Andrews.

(*With Portrait on Special Plate.*)

SIR JAMES MACKENZIE died at his residence in London on the morning of January 26th. He had been suffering from angina pectoris for some time, and about a year ago decided not to spend another winter in St. Andrews, where he had gone to live after leaving London in 1918. At St. Andrews he had a delightful house and garden, and there he founded the Institute for Clinical Research, to which he gave a great part of every day while his health permitted. With him English medicine loses one of its great names. For over a quarter of a century he was in general practice in Burnley, and it was during those years that he began and carried far forward the investigations which were to revolutionize cardiology. He attained a position of world-wide eminence as a research worker and clinical investigator in what is admittedly a difficult subject. His success was due to a buoyant temperament which, coupled with untiring industry, enabled him to get through an immense amount of work; to a well founded belief in his own capacity to observe and reason correctly; and to an innate mental fearlessness and integrity which compelled him to rest his doctrine on facts rather than on authority.

He was born in Scone, the ancient coronation place of the Scottish kings, on April 12th, 1853; he was educated at Perth Academy and at the University of Edinburgh, where he graduated in medicine in 1878, and received the degree of Doctor of Medicine in 1882. After a term spent as house-physician in the Edinburgh Royal Infirmary immediately following graduation, he began practice in Burnley in Lancashire, and remained there for twenty-eight years. It was during his residence in that town, and while engaged in all the activities of a busy general practitioner, and with a hospital appointment, that he carried out the investigations which led to the introduction of the polygraph as a means of studying the various irregularities of the pulse. Perhaps during no part of his life is his character as a painstaking, persevering investigator better brought out. He says in the preface to one of his publications that amid the distractions of his daily work he has "seldom been able to give an uninterrupted hour's study to the subject." There is no doubt, on the other hand, that much of his research could not have been undertaken otherwise than in general practice, for many of his conclusions were arrived at after following the course of disease in individual patients over many years.

Mackenzie early turned his attention to the investigation of the interpretation of symptoms, and to the elucidation of the various irregularities in cardiac rhythm. We find him in 1892 contributing to the *Medical Chronicle* a paper on "The study of sensory symptoms associated with visceral disease," and two years later, to the *Journal of Pathology and Bacteriology*, a paper on "The venous and liver pulses and the arhythmic contraction of the cardiac cavities"; but it was in 1902, when his book on *The Study of the Pulse* appeared, that Mackenzie became definitely recognized as one who had produced something which was likely to revolutionize our ideas of cardiac irregularities, and we find this book translated into German, in which country his work was already attracting attention.

In 1907 he settled in London and quickly took his place as one of the foremost consultants in diseases of the heart. He was appointed physician to the Mount Vernon Hospital for Diseases of the Chest, and some years later (about 1913) became physician in charge of the cardiac department at the London Hospital.

In 1908 there appeared his *magnum opus* on *Diseases of the Heart*, which, in the course of five years, went through three editions, and was translated into German, French, and Italian. When war broke out his wards were utilized for military purposes, and he had perforce to give up his regular teaching, though he continued his consulting practice in London till 1918. During his residence in London he was the recipient of many honours. In 1911 he delivered the Oliver-Sharpey Lectures and also the Schorstein Lectures—in the former dealing with heart failure and in the latter with auricular fibrillation. In 1914 he was appointed first George Alexander Gibson Memorial Lecturer at the Royal College of Physicians, Edinburgh, and in 1915 he was elected Fellow of the Royal Society for his researches into the heart in health and disease. In the same year he received the honour of knighthood from His Majesty, and a few years afterwards was appointed honorary consulting physician to the King in Scotland. During this period he also produced his shorter work on the *Principles and Diagnosis of Treatment in Heart Affections* and in 1909 a book on *Symptoms and their Interpretation*, which passed through four editions and was translated into four European languages.

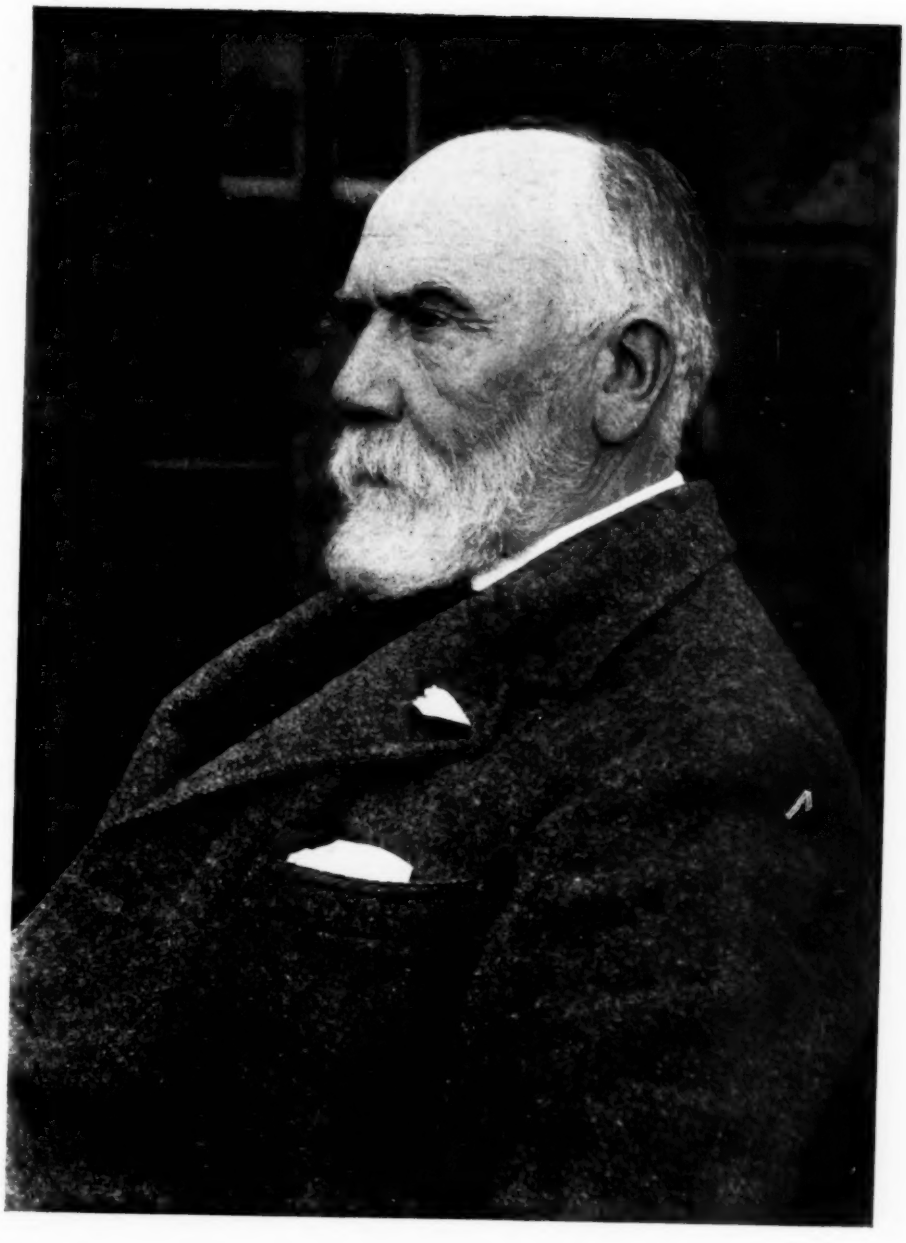
Following his resignation from the London Hospital his work took on a new aspect, and the publication in 1919 of his book on *The Future of Medicine* raised a considerable amount of discussion. His aim was to encourage research in the beginnings of disease, and with this object in view the St. Andrews Institute for Clinical Research was founded, and to it Sir James Mackenzie devoted the remaining years of his life.

Many of his most important contributions to periodical medical literature, both during his residence in Burnley and afterwards, were published in this JOURNAL; the most recent were "A new outlook in cardiology," a long paper which ran through three numbers and dealt in particular with the sino-auricular node, the auriculo-ventricular node, auricular flutter, and the cause of heart failure; a "Preliminary inquiry into the nature of the cell impulse"; and his last contribution, "A plea for clinical physiology," all published during 1924. In addition to the papers in this and other medical journals he had published in recent years two books—one on *Heart Disease and Pregnancy*, and the other, the most important of his later works, on *Angina Pectoris*, which was the result of his investigations into this subject from the time of his early days in Burnley.

Born in Scotland, of Scottish parents, Mackenzie never lost the dogged determination that is said—not without truth—to be characteristic of that nation. Endowed with an inherent inclination to surmount difficulties, he manifested that form of genius which is defined as being "the infinite capacity for taking pains," but he was also possessed of vision and was not content to confine himself to the broad road. In his later years he concentrated his energy on stirring up a righteous discontent in the profession with things as they are, and, adopting the prophetic mantle, aimed at showing it a more excellent way.

The magnitude of the work he did in connexion with disease of the heart has been recognized all over the globe. He was early impressed with the inability to interpret the different irregularities of the pulse, and out of the chaos of the nineteenth century he evolved the order of the twentieth. Had he done nothing more than isolate the heart with auricular fibrillation from other conditions, and demonstrate its relation to digitalis therapy, he would have placed the profession under a lasting debt of gratitude. His work with the polygraph may be said to have revolutionized our knowledge of the cardiac mechanism and put the diagnosis and treatment of certain diseases of the heart on a logical basis, while it laid the foundation upon which most of the more recent work has been done. His views on cardiac failure are now so generally accepted that it is already difficult to realize that he put a new complexion on this aspect of the subject, and if some may have thought that he took an extreme position it must be remembered that it required strong dogmatic teaching to arouse a self-satisfied profession.

Throughout the course of his research on heart disease he was constantly met with difficulties from his inability to interpret many simple symptoms, and he was thus led into bypaths which he has illuminated in his earnest endeavour to seek out the truth. Perhaps nowhere is his thoroughness and his determination to get first-hand information better illustrated than when he describes in his book *Symptoms and their Interpretation* an operation at which he was the



J. Mackenzie

surgeon: "I had occasion to resect a small portion of the small intestine in a conscious subject, for umbilical fistula, whose abdominal cavity I laid open. He refused to take an anaesthetic, and no analgesic, local or general, was administered"; and then follow the results of his observation, illustrating his views on the reflex character of visceral pain. He considered that the taking of surgery out of the hands of the general practitioner was a great loss to medicine, and he maintained that if practitioners could not themselves perform an operation on a patient they should at least be present to find out the explanation of the symptoms.

He long held the view that medicine was taught in the wrong order, and that the simpler and commoner diseases should be demonstrated by the out-patient department, while a junior physician would be quite competent to teach in the wards on the fully developed diseases.

When he retired from consulting practice in London he had already prepared his plans for an Institute of Clinical Research, and in St. Andrews he found an ideal centre, with a comparatively stationary resident population, for putting into practice his views regarding the study of the beginnings of disease. He realized that it would be many years before the Institute could prove its real value, but he hoped to have the opportunity of training others in the method he considered best adapted for the purpose in view. It is yet too soon to estimate the full results of this scheme, but under his directorship many valuable contributions to clinical medicine have already appeared in its published reports, while the practitioners in St. Andrews who are on the staff of the Institute acknowledge the very great assistance it has been to them in their practices, and the inspiration and interest in their work which contact with the "Chief" has engendered. He had the power of inspiring enthusiasm among his co-workers, and he never lost an opportunity of preaching his principles to any visitors who came to the Institute. In his presence one could not fail to be affected by the magnetism of his personality. When carried away with his own views, whether in conversation or in writing, he was apt to leave the impression that other lines of investigation than those adopted by him were useless, and in this way his diatribes against stethoscopes, electro-cardiographs, valvular murmurs, and morbid anatomy were apt to be misleading to those who did not understand him. For all of these things, however, he found a useful place in his own writings, and his aim was to prevent others from giving them an undue prominence in their medical work.

He was probably not at his best with a large audience. The attention required for careful diction was liable to curb the flow of his thoughts, and he was apt to drop into a more colloquial and forceful manner of address. He was at his best with a few listeners, and could always display an earnestness and sincerity which never failed to impress.

Kindly in disposition and always approachable, though sometimes almost brusque in manner, he never lost an opportunity of trying to inculcate his views on the many medical men who visited him from all parts of the world. Though it may be said that he lived for his work, he found time to have a daily round of golf until latterly he found the strain too much. His health had been failing for some time, but even when his attacks of pain were becoming more frequent and more acute he still found time to devote to his work. Like not a few men who have risen to eminence in the medical world, he was cut off by an affection of that organ the functions of which he had done so much to elucidate.

In 1887 he married the daughter of Dr. George Jackson of Boston; two daughters were born to them. To Lady Mackenzie, his devoted wife, and to her surviving daughter, from both of whom he received untiring encouragement and help in his happy home life, the sympathy of the profession and their large circle of friends will go out in their grievous loss. His brother is Sir William Warrender Mackenzie, K.C., K.B.E., one of the editors of Lord Halsbury's *Laws of England*.

The portrait with which this notice is illustrated is from a photograph taken at the Institute for Clinical Research, St. Andrews, by Mr. Smith. We are indebted for it to Professor Waterston.

TRIBUTE BY SIR THOMAS LEWIS.

We are indebted to Sir Thomas Lewis for the following tribute to Sir James Mackenzie's character and work:

It was in 1908, and shortly after he migrated to London from his old home and practice at Burnley, that I first came to know Mackenzie personally. While it was never my privilege to work under his supervision, yet he extended to me, though many years his junior, a friendship which soon became intimate and highly prized. I found in him during those years before the war, when we were in almost daily and personal communication, a man whole-heartedly devoted to the search for knowledge of disease, and eager, most eager, to impart his spirit and conceptions. He was an exceptionally vigorous and strong personality, intolerant of statements founded on tradition, trenchant and acute in criticism, rich in personal experience, combative in argument, but open nevertheless to conviction on all questions without reserve. He saw, as few or none of his day saw, where clinical knowledge ends and ignorance begins, and never hesitated to define the limits of his knowledge; he was the first authority in clinical medicine on whose lips I frequently heard the words, "I don't know." To him diagnosis, the affixing of a contemporary label, gave little satisfaction; his patient still contained his problem or his problems. Mackenzie's mind could not rest on the known, but turned incessantly, at each and every hour of the day, to the unknown, and in perceiving and defining the unknown he displayed a masterly power. To know Mackenzie intimately was to imbibe independency of thought; none could go to him seriously to discuss and, surviving the brusque shocks of that first interview, return without a growing inspiration and stimulus. For he possessed the gift of stimulating others in unusual degree.

As a man he was hard-headed and warm-hearted, quick in temper and in sentiment, genial and hospitable, possessed of an invigorating blunt humour. Schooled by long experience in the sensibilities, reservations, obstinacies, and frailties of patients, he was an astute and shrewd physician, who rarely failed to inspire an unlimited and merited confidence.

This briefly of his practical work as I knew it; as a bedside investigator he ranks, in my judgement, as the first of his time; he has gone from us to hold a place in the history of British medicine by the side of Sydenham, Stokes, Graves, Addison, and Bright, and he will live in memory as not the least of such men. To describe the solid work accomplished by him in his long, arduous, but always enthusiastic struggle for "the light," to attempt to weigh its detail and to assess its breadth, not in one but in its many phases, fully and with truthfulness, cannot be done briefly; to summarize that which most of his contemporaries in large part know—for his light burned brightly and unconcealed—would, I think, belittle the greatness of his achievement.

To the end of a long and painful illness, when the hand of that malady which John Hunter also suffered, and into which none had a clearer insight than Mackenzie, was heavily upon him, his mind retained all its former alertness, searching his own bodily symptoms, and not unsuccessfully, for some further clue to their meaning; this last concentration was to him inevitable, being but a natural epilogue to the drama of a full and rich life.

A TRIBUTE FROM ST. ANDREWS.

In the deep sense of recent personal loss it is not easy for his St. Andrews colleagues to pay an adequate tribute to the memory of Sir James Mackenzie, and it is possible only briefly to mention some aspects of his work and his influence there.

Sir James Mackenzie's work culminated in his foundation of the Institute for Clinical Research at St. Andrews. The reasons which induced him to establish the Institute there and the special line of research which he desired the Institute to represent have been fully expounded by himself in various papers and in the reports of the Institute. As a clinical investigator his work began with the observations which he made while in practice at Burnley. His busy consulting and hospital practice in London added to the wealth of his experience, but it was in his work at the St. Andrews Institute that he summed up and put forth

the considered conclusions drawn from the clinical observations made during the course of his life's work.

The conception of an Institute devoted to clinical investigation was the outcome of his pioneer work as an investigator while a general practitioner. He formulated the fundamental truth that medicine is a special branch of science dealing with a special group of phenomena to which the clinical investigator must apply the methods specially suitable for his purpose. Other sciences have their contribution to make to medicine, but they are to be regarded as ancillary, and not replacing the proper methods of clinical investigation into the phenomena of ill health and disease which afford the material for study. Among other things, it was essential, in his view, to employ the opportunities the general practitioner alone possesses of watching the "life-history" of ill health in patients over a long course of years.

St. Andrews was known to him, was a place of convenient size, and seemed in other respects suitable for launching his great enterprise. After paying some visits to it he decided to give up his appointments and his work in London and to devote his energy and ability to this new field.

What it meant to leave London and his work there is not easily estimated. The London Hospital authorities had given him every facility for special hospital work. His position had been recognized as a great consultant, clinician, and teacher. He deeply appreciated the facilities which had been afforded him, both at the London Hospital and at the Mount Vernon Hospital, Hampstead. But he realized that he could not in London carry on the work which he felt was fundamentally necessary for the advance of medicine, and after full consideration he decided to leave London and to devote himself to founding a research institute.

He came to St. Andrews unknown to all but a few of his medical colleagues, and began at the Cottage Hospital a series of clinics in the out-patient department, to which he soon attracted all the practitioners. These clinics rapidly produced the desired effect and aroused in his audience the desire to co-operate in the work which he showed them was waiting to be done. With the assistance of generous friends who were deeply interested in his ideas, and with his own financial help, a suitable building was secured and fitted up, and the definite work of the Institute was begun. At a later stage, as the usefulness and value of the work became realized, additional help was given by the Carnegie Trust and by the Medical Research Council.

Of the work which has been accomplished at the Institute this is not the occasion to write at length, nor is it possible as yet fully to estimate the significance and the far-reaching results of this pioneer movement in medicine. It has already had an influence far wider than a merely local one, and medical thought and medical practice in many countries of the world have received from it direction and guidance. The recent award to him of the Mickle Fellowship of the University of Toronto is an illustration of the wide recognition given to his work in St. Andrews.

For three years he devoted his whole time and thought to the work of the Institute and to the training of those who worked with him there, and then, satisfied that he had laid down the lines for their work, he gradually withdrew himself from active direction, and gave to the members of the Institute the opportunity of carrying on the work by themselves. At a still later stage his health began to fail, and he decided to winter in a milder climate, but to the very last the welfare of the Institute was foremost in his interests and thought.

It is not easy for his colleagues to realize fully the extent of the loss they have sustained by his sudden death. His vivid personality and the vigour of his mind had made a deep impression upon them and the stimulus of coming into contact with him in discussion and debate was of incalculable benefit to them all. They realized the greatness of his genius and his extraordinary insight into medicine, and they had come to look upon him, not only with respect and admiration, but with the most sincere affection.

Great as was the influence of his personality his colleagues feel that the value of his work did not depend upon that element, and will not end with his death. Not only in his work upon the heart, but in the wider field of general medicine, his observations and the principles which he has deduced from them are of permanent and abiding value.

SIDNEY BARWISE, M.D., B.Sc., D.P.H.,

Medical Officer of Health for the County of Derby.

THE death of Dr. Sidney Barwise on January 24th came as a great shock to his large circle of friends and to the public health branch of the profession generally. Although for some little time his health was giving cause for anxiety, his mind still retained its remarkable activity, and his energies seemed unabated in devising schemes for the benefit of humanity. His position amongst medical officers was in some respects unique, and his loss will be very real.

Sidney Barwise studied medicine at Queen's College, Birmingham, and obtained the L.S.A. in 1884 and the M.R.C.S. diploma in 1885. Two years later he graduated M.B.Lond. and proceeded M.D. (in State medicine) in 1893. He obtained the B.Sc. of Birmingham University in 1904. He had commenced his public health career in Blackburn in the year 1889. Although he only held the post of medical officer of health for that town for two years, he organized its sanitary administration on sound lines and conducted important investigations into two pressing problem—the smoke nuisance and the health of cotton operatives. Although the foundations of his reputation as a public health worker were laid whilst in Blackburn, it was as the medical officer for the county of Derby that his life's work was done. During the earlier years, when the powers and duties of county councils were much more restricted than they now are, his energies were more particularly directed to the provision of isolation hospitals for infectious disease and the prevention of river pollution. In both these branches he did pioneer work.

A colleague in the public health service writes: Barwise's real opportunity occurred with the extension of the duties of county councils to personal preventive medicine under the schemes of medical inspection of school children, maternity and child welfare, prevention and treatment of tuberculosis and venereal diseases, and the Midwives Act. His work under these schemes has not only proved to be of great value to his county, but has often been a real inspiration to his fellow workers in other parts of the country. His scheme for the dental treatment of school children was perhaps the most complete of any county scheme. It embodied the controversial procedure of employing dental dressers, but in his opinion this was the only method at the present time by which the great mass of school children of the country could get efficient treatment. He may have been right or wrong, but it was characteristic of him that, having come to this conclusion, he pursued his course in face of great opposition, buoyed up with the intense conviction that he was acting in the best interests of humanity. Barwise had the public health "sense" very strongly developed, and his fertile mind was almost continually engaged on schemes, many practicable, some impracticable, for the betterment of the people. As showing that his interests were not entirely confined to his work, it may be mentioned that a letter written in 1912 forecasting the war and the details of invasion of Belgium and France proved remarkably correct in detail and showed that he was a student of contemporary history. Notwithstanding his long and valuable service, one cannot help feeling that by his death a career has been cut short which still had much promise. Dr. Barwise leaves a widow and two children.

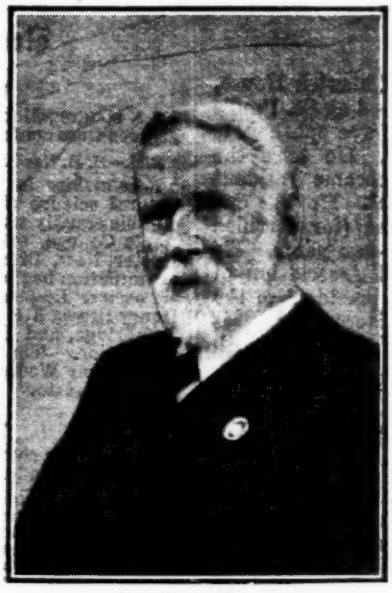
Another colleague writes: To any member of the public health service who was privileged to know Dr. Barwise his sudden death will come as a great shock and cause real grief. His infectious enthusiasm, his keenness to acquire knowledge, his energy, which seemed never to flag, and his generosity and kindness, will always be treasured memories to his staff. During the long period he was county medical officer for Derbyshire there was an unceasing

output of work, each of his annual reports, always so stimulating and interesting, showing some fresh achievement. And now that he has laid down the reins there remains as a record a public health department which must be a great source of pride to the Derbyshire County Council and the envy of others.

JOHN FRASER, M.B., C.M., F.R.C.P.Ed., F.R.S.Ed.,

Retired Commissioner, General Board of Control for Scotland.

THE death of Dr. John Fraser at his residence in Edinburgh on January 18th removes a notable figure in Scottish lunacy affairs. Dr. Fraser, who was in his 80th year, recently met with an accident when alighting from a tramcar, which rendered him unconscious, and, although he lingered on for some days, the shock at his advanced age proved fatal.



DR. JOHN FRASER.

John Fraser was born at Thornhill, Dumfriesshire, where his early years were spent. He studied medicine at Edinburgh University, where he had a distinguished record, gaining nine medals in his medical classes, and he graduated there as M.B., C.M., with first class honours, in 1870. After qualifying, he held resident appointments in Edinburgh Royal Infirmary, and thereafter his attention was directed towards the study of mental diseases. In 1871 he was appointed assistant medical officer at Fife District Asylum, Cupar, and a few years later he succeeded the late Sir John Batty Tuke as medical superintendent. The ties of friendship between those two distinguished alienists were later drawn closer by the marriage of John Fraser to Batty Tuke's daughter. In 1877 Fraser was appointed a Deputy Commissioner in Lunacy. As a Deputy Commissioner Fraser's duties were mainly concerned with the insane living under private care, and into this work he entered with heart and soul, and with a determination to develop and extend more thoroughly the system of boarding out harmless patients in private dwellings under suitable guardianship, a scheme which had been initiated by his predecessor, Sir Arthur Mitchell. His energy and enthusiasm soon gained the entire confidence of inspectors of poor and other officials, and animated many of them with his conviction as to the advantage of such a system both to the ratepayer and as a measure of justice to the many insane who, under slight supervision, are capable of enjoying the liberties and pleasures of a natural home life. The system of boarding out suitable cases became generally adopted throughout Scotland, and it has continued to be a marked feature of Scottish lunacy administration. In 1895 Fraser was appointed Commissioner in Lunacy, and in his new capacity soon gained the confidence and esteem of medical superintendents and other asylum officials. The superintendents of the Scottish asylums soon found that he was a friend who understood their many difficulties and anxieties. He was regarded by them less as an official critic than as a friendly and outspoken adviser whose mature experience and knowledge were always at their service—one who sympathized with them in their difficulties, and was always ready to do all in his power to assist them both with kindly counsel and active support.

Fraser's official connexion with lunacy—extending over thirty-nine years—covered a period of general progress in every direction in the treatment of the insane. There was no method of treatment of recognized merit which escaped his notice or failed to receive his warm approval and support; once convinced that it was for the benefit of the insane he did his utmost to influence others to adopt it. He was thus a real and active force in promoting the most progressive views. His name is bound up with the history of the treatment of the insane in Scotland, and his period of office as a Commissioner was a most fruitful one and left an indelible mark on the internal administration of asylums, and through his efforts he greatly assisted to improve the conditions for patients in the Scottish asylums. Among the advances initiated during his time may be mentioned particularly the hospitalization of asylums, with improved and more efficient systems of nursing the insane both by day and by night. He was a strong advocate of all non-restraint methods of treatment, and also of the nursing of male patients by female nurses, which now characterize the administration of Scottish mental hospitals. He was largely instrumental in having precautions against fire perfected in all asylums, and in having efficient means provided for the safety of the patients.

His kindly manner and cheery optimism earned for him the respect of both patients and staff as a genuinely sympathetic visiting Commissioner to whom appeals were never made in vain. His kindly words of advice comforted many and helped to smooth difficulties and dispel misunderstandings, the patients feeling that in him they had a real friend, who took a genuine interest in their welfare. As a Government official and public servant he was highly respected, and he worthily maintained the traditions of his office. Dr. Fraser, on his retirement from the General Board of Control under the age limit in 1910, was publicly presented by his numerous friends and admirers with his portrait painted by Mr. Fiddes Watt, R.S.A. Since then he had devoted his time and energies to charitable organizations, more particularly the Society for the Prevention of Cruelty to Children, in which he was keenly interested. He was a director of the Royal Scottish National Institution at Larbert for the care of the feeble-minded. He was predeceased by his wife in 1917, and he is survived by three sons, one of whom is an ear and throat specialist in Edinburgh, and two daughters. A memorial service was held in St. Cuthbert's Parish Church, Edinburgh, on January 21st, at which representatives from various public bodies were present, with many personal friends. The interment was in the Dean Cemetery, Edinburgh.

We regret to announce the death of Dr. GEORGE COOKE ATTFIELD, on January 16th, within a few days of his 99th birthday. Dr. Attfield was born at Bath on January 27th, 1826, and was educated at St. Bartholomew's Hospital; he obtained the diploma of M.R.C.S.Eng. in 1850. For some years he was medical officer of Millbank Prison, and he subsequently became chief medical officer of prisons in Western Australia, where he remained until the demolition of the convict settlement in 1879. He then returned to England and went to live at Hove. From his early youth he was a keen sportsman, and was especially distinguished as a cricketer. He played at one time in the Somerset XI, and it is said that when in Australia he was invited to join an All-England team touring in that country, but was unable to accept. He continued to take the keenest interest in county cricket until his death, and was also well known as a billiard player. Dr. Attfield could remember the epidemic of cholera in London in 1842-3, and during the London bread riots of 1848 he served as a special constable. At the time of his death he was believed to be the oldest medical practitioner in England.

By the death of Dr. OWEN BOWEN, which took place on January 19th, Liverpool loses one of its best known practitioners in the North End. Dr. Bowen had been far from well during the past two years, but with characteristic fortitude continued to practise till within a few weeks of his death. He qualified in 1876, taking the usual qualifications

at that time of M.R.C.S. and L.S.A., and shortly after settled down to practice in Liverpool. He was for many years an active member of the Lancashire and Cheshire Branch of the British Medical Association, and as a Divisional Representative on the Branch Council was noted for his punctuality and assiduity in attendance at its meetings. He was honorary anaesthetist to the Liverpool Eye and Ear Infirmary, and as a colleague was much appreciated for his quiet, thoughtful demeanour. Not given to public speaking, he was one of those men who believed he could do more good by persuasion and example, and thus his influence was felt among his professional brethren. The funeral took place on January 22nd at Anfield Cemetery, where many friends and patients attended to pay their last tribute. Dr. Owen Bowen was a widower, but in his last illness was tenderly cared for by his stepdaughter, to whom and his other relatives we desire to express sympathy.

Dr. CHARLES MOLESWORTH TUKE died at Chiswick House, Chiswick, on January 24th. He was educated at St. Paul's School and St. George's Hospital, where he was ophthalmic assistant and assistant surgical registrar. He took the diploma of M.R.C.S.Eng. in 1881, and became resident clinical assistant to Bethlem Royal Hospital. He was one of the medical licensees of Chiswick House (for certified patients and voluntary boarders) for thirty-two years. He was a member of the Medico-Psychological Association of Great Britain and Ireland, and in 1904 was president of the West London Medico-Chirurgical Society. Recently he was a member of the Government Committee for dealing with mental derangement among discharged sailors and soldiers. He was, in his youth, a keen cricketer, and is remembered as a bowler. Though he took no active part in politics, he was a ruling councillor of the local Habitation of the Primrose League.

Dr. EDWARD REGINALD TWEED died at his residence, near Honiton, Devon, on January 9th, at the age of 62. The son of the late George Tash Tweed, a solicitor of Honiton, he was educated at St. George's Hospital, where he obtained the L.S.A. in 1885, the M.R.C.S. and L.R.C.P. diplomas in 1887, and the M.D.Brux. degree in 1888. Dr. Tweed practised for many years in Grosvenor Square, London, and was late coroner for East Devon. During the war he served as registrar to the University War Hospital at Southampton in 1916, and afterwards for two years in the 2nd Western General Hospital in Manchester. He was a member of the Honiton Board of Guardians and Rural Council. He regularly hunted with the East Devon Hounds and Axe Vale Harriers.

Dr. ARTHUR G. CHEYNE IRVINE died at Wellington College, Berks, on January 20th after a long illness contracted during war service as a temporary captain R.A.M.C. Born at Coleraine, Ireland, in 1864, he was educated at King Edward's Grammar School, Birmingham, whence he passed to Queen's College, Birmingham, to study medicine, obtaining the diplomas of M.R.C.S.Eng., L.R.C.P.Lond. in 1890. After a year as assistant house-surgeon to the West Bromwich Hospital he set up a practice at Selly Oak, Birmingham, where his clinical acumen, keenness, and sympathy soon won the esteem of a large number of patients and fellow practitioners. For some years he acted as general secretary to the Birmingham and District General Practitioners' Union. In 1914 he was actively engaged in examining recruits, and in 1915 received a commission in the R.A.M.C., serving at Salisbury Plain, and then at the Command Depot, Sutton Coldfield. He was promoted to captain in 1916, but was invalided out of the service in July, 1917, and with great fortitude and patience lived through eight years of ill health in retirement, leaving a widow and one daughter. Fond of reading and a really good chess player, representing his county in tournaments, Dr. Irvine bravely adapted himself to the limitations imposed by his ill health, and those who knew him were filled with admiration at his courage and feel that his death is a very real loss to the medical profession.

Dr. JOHN HENRY MONTGOMERIE BELL, who died on December 27th, 1924, at the age of 47, was educated in Edinburgh, where he graduated M.B., Ch.B. in 1901, and proceeded M.D. in 1907; he obtained the diploma F.R.C.S.Ed. in 1907 also. His appointments included house-surgeon to the Deaconess Hospital, Edinburgh, house-surgeon to the eye wards, and house-physician to the Edinburgh Royal Infirmary, and senior house-surgeon to Rotherham Hospital. Dr. Bell served with the Canadian Army Medical Corps throughout the war, reaching the rank of major.

Dr. PAUL DELBET, the well known surgeon of the Necker Hospital, Paris; Dr. ZEFFERINO FALCAO, professor of dermatology at Lisbon; and Dr. PIETER VAREKAMP, a well known Dutch balneologist, have recently died.

Universities and Colleges.

UNIVERSITY OF OXFORD.

AT a congregation held on January 22nd the following medical degrees were conferred:

B.M.—H. E. Harding, A. W. L. Row, M. J. W. Minshull.

Scholarships in Natural Science.—A number of scholarships in natural science (for which intending students of medicine are eligible equally with others) are offered for competition as follows: Keble College, examination beginning on March 16th; Merton, Exeter, New, Brasenose, Corpus Christi, and Wadham Colleges, examination beginning on March 17th.

Romanes Lecture, 1925.—This will be delivered by Sir William Henry Bragg, K.B.E., D.Sc., F.R.S., Hon. Fellow of Trinity College, Cambridge, and Fullerian Professor of Chemistry at the Royal Institution. The lecture will be delivered in the Sheldonian Theatre on Wednesday, May 20th, at 5.30 p.m.; subject: "The crystallin state."

UNIVERSITY OF LONDON.

THE following course of lectures has been arranged at the colleges indicated, and admission to them is free:

Middlesex Hospital Medical School.—Eight lectures on the chemistry of the internal secretions by Dr. E. C. Dodds, on February 3rd, 5th, 10th, 12th, 17th, 19th, 26th, and March 3rd, at 5 p.m. Attendance at this course is recognized in connexion with the B.Sc. (Honours) degree in physiology.

St. Bartholomew's Hospital Medical College.—Four lectures on the medullary centres by Dr. J. M. Duncan Scott, on February 11th, 18th, 25th, and March 4th, at 5 p.m.

CONJOINT BOARD IN SCOTLAND.

THE following candidates have been approved at the examination indicated:

FINAL EXAMINATION.—*Medicine:* F. D. Muller, S. W. Tay, W. A. Kemp, R. T. Johnson, A. Chellappa, S. Ramakrishnan, N. K. Menon, J. Houston, A. A. F. Shepherd, J. Paterson, J. S. Whiteside, A. B. Jayasuriya, J. A. Hyde, M. L. Hendrie, M. M. El-Makkawi, M. J. Quraishi, G. F. Maher. *Surgery:* H. B. Wainer, A. Wilson, F. D. Muller, A. E. Williamson, M. Kopelowitz, R. T. Johnson, S. Ghaleb, S. Ramakrishnan, J. Harkness, A. A. F. Shepherd, R. G. V. Dias, J. S. Whiteside, L. K. Htoe, M. E. T. Burke, M. M. El-Makkawi, H. Goldsmith, J. C. Stenhouse, M. J. Quraishi. *Midwifery:* J. R. Hetherington, H. B. Wainer, D. D. H. Craig, S. W. Tay, J. S. Amarasingham, C. K. Dowson, N. K. Menon, D. L. Crawford, J. Houston, J. Harkness, A. B. Jayasuriya, J. A. Hyde, L. K. Htoe, C. Ismail, M. L. Hendrie, H. Goldsmith, A. C. Young. *Medical Jurisprudence:* H. D. J. Lazarus, R. A. B. Leakey, B. J. F. Laubscher, D. Lumsden, C. A. S. Hamilton, M. M. Bronstein, W. E. Williams, Elizabeth C. Mudie, R. T. Johnson, B. S. Ellis, N. Valerio, A. S. Pool, A. Rankin, J. A. Mains, S. K. Mitra, C. L. Tan, W. Jackson, W. C. Heslop.

The following twenty-nine candidates, having passed the Final Examination, have been admitted L.R.C.P.Edin., L.R.C.S.Edin., L.R.F.P.and S.G.:

J. E. Israel, W. A. D. Oliver, V. Vaithialingham, P. J. Badenhorst, E. A. M. McKinney, Florence M. Smith, H. S. Foo, G. E. M. Scott, Kathleen M. R. Dunn, N. Finn, K. Somaskander, K. K. Nayar, N. T. Yusif, H. Krafchik, E. G. Douglas, J. M. Rutherford, A. M. Filer, R. W. Crossette-Thambiah, N. Wren, P. A. M. Jayewardene, J. C. Shiach, I. Michaelson, H. A. Newton, A. Vasudev, Mollie F. Churcher, V. V. Narasimham, Eleanor P. Topping, J. T. Smith, W. M. B. Possie.

SOCIETY OF APOTHECARIES OF LONDON.

THE following candidates have passed in the subjects indicated:

SURGERY.—A. J. Dowek, T. McD. Kellough, J. Maingard, E. J. P. McDowell, †A. Mishriky, B. L. Steele, C. C. Taffs.

MEDICINE.—W. Bentley, *C. V. Brown, N. E. Challenger, L. Freeman, E. F. Hottinger, *R. C. Pratt, C. W. Warne, D. Winstanley.

FORENSIC MEDICINE.—N. E. Challenger, E. F. Hottinger, W. Johnson, W. I. Pierce, R. J. Rutherford, H. Winstanley.

MIDWIFERY.—M. R. Burke, F. Carroll, C. E. Hagenbach, E. F. Hottinger, A. J. Moody, K. E. R. Robertson, H. Winstanley.

* Section I. † Section II.

The diploma of the Society has been granted to Messrs. A. J. Dowek, L. Freeman, E. F. Hottinger, E. J. P. McDowell, A. J. Moody, C. C. Taffs, and C. W. Warne.

The Services.

DEATHS IN THE SERVICES.

Colonel Robert James Copeland, Army Medical Staff (ret.), died in a nursing home at St. Leonards on January 11th, aged 62. He was the son of the late Major Copeland, of the Indian army. He was educated at Edinburgh, where he graduated M.B. and C.M. in 1886, and entered the army as surgeon on July 27th, 1887. He became lieutenant-colonel on January 30th, 1912, was on temporary half-pay on account of ill health from March 10th to September 10th, 1912, and was promoted to colonel in the big war promotion list of March 1st, 1915. He served throughout the South African war of 1899-1902, when he took part in the relief of Kimberley; in operations in the Free State, including the actions at Paardeberg, Poplar Grove, Dreifontein, Karee Siding, and Zand River; in the Transvaal, actions near Johannesburg, at Pretoria, Diamond Hill, and Belfast; and in Cape Colony, in the action at Colesberg; and received the Queen's medal with six clasps and the King's medal with two clasps. He also served throughout the recent great war as A.D.M.S. of a division, and in command of a general hospital. He retired on August 10th, 1919.

Lieut.-Colonel Delaware Lewis Irvine, R.A.M.C. (ret.), died on January 14th, aged 71, in Westminster Hospital, of injuries caused by being knocked down by a motor lorry when crossing Whitehall on January 7th. He was born at Jesmond, Newcastle, and entered the army as surgeon on July 31st, 1880, became lieutenant-colonel after twenty years' service, and retired on March 28th, 1908. After retirement he was employed from 1909 to 1913 as staff officer to the A.M.O., North Midland Division, Territorial Force. He served in the Sudan expedition of 1884-85, in the Nile campaign, when he was present at the action of Kirbekan, and received the medal with two clasps and the Khedive's bronze star; and in the South African war in 1899-1900, when he took part in the relief of Kimberley; in operations in the Orange Free State, including the actions at Paardeberg, Poplar Grove, and Dreifontein; in those in the Transvaal, with the actions near Johannesburg, Pretoria, and Diamond Hill; and in Cape Colony, in the action at Colesberg; and received the Queen's medal with five clasps.

Major Arthur George Edward Newland, Madras Medical Service (ret.), died in Edinburgh on December 28th, 1924, aged 67. He was the son of the late Charles Edward Newland, schoolmaster, Coimbatore, was born in India, and educated at the Madras Medical College. After completing his medical course he served for a short time as a civil assistant surgeon in the Madras Presidency, in 1879-80. Coming home, he took the L.R.C.P. and S.Ed. in 1880, and entered the I.M.S. as surgeon on April 2nd, 1881, becoming major after twelve years' service, and retiring on August 1st, 1899. He served at Suakin in the Sudan campaign of 1885; in Burma in 1886-89, receiving the frontier medal with a clasp; in the Manipur campaign of 1891 (clasp); and again in Burma from 1891 to 1896, taking part in operations in the Chin Hills and on the North-East frontier, with the Baungshe, Tlang Tiang, and Tashon columns (clasp). He was the author of *The Image of War, or Service in the Chin Hills* (1894); and a *Handbook of the Languages of the Luis* (1897).

Medical News.

As will be seen from our advertisement pages, applications for pensionerships and foundation scholarships at Epsom College must be sent in by the morning of February 18th. An examination is also about to be held for the admission of boys as council exhibitioners at the College, applications for which must reach the office, 49, Bedford Square, W.C.1, by the morning of February 18th.

THE annual dinner of the Hunterian Society will be held at the Hotel Victoria, Northumberland Avenue, on Thursday, February 12th, at 7.30 p.m. The Lord Mayor and Lady Mayoress of London, the Bishop of London, and several other distinguished guests will be present. The price of the dinner is 12s. 6d. (exclusive of wines). Decorations will be worn.

THE second annual evening concert, given by the London Stock Exchange Male Voice Choir in aid of the Royal Medical Benevolent Fund Guild, will be held at Wigmore Hall, Wigmore Street, W., on February 24th, at 8.15 p.m. The choir will be assisted by Miss Phyllis Lett, Mr. Edward Halland, Miss Harriet Cohen, solo pianoforte, and Mr. Leonard Dove, accompanist. Tickets—price 10s. 6d., 7s. 6d., and 5s. reserved; unreserved, 2s. 6d.—may be obtained from the box office, Wigmore Hall, or from the honorary organizer, Mr. C. Godwin, 10, Drapers Gardens, E.C.2.

AT a meeting of the Royal Sanitary Institute, to be held at 90, Buckingham Palace Road, S.W.1, on Tuesday, February 10th, a discussion on food and health will be opened by Major-General Sir Wilfred Beveridge, K.B.E., who will be followed by Dr. W. M. Willoughby. The chair will be taken at 6 p.m. by Dr. Louis C. Parkes.

DR. MAUD F. FORRESTER-BROWN, M.S., has been appointed for a third year to the William Gibson research scholarship of the Royal Society of Medicine for research in orthopaedics.

THE Fellowship of Medicine announces that Mr. Arthur Giles will lecture, on February 2nd, on the operative treatment of uterine displacements, at 1, Wimpole Street, at 5.30 p.m. During February the following courses will be held: a three weeks' combined course in diseases of children at the Paddington Green Hospital, Victoria Hospital, and the Children's Clinic; a four weeks' course in dermatology at the St. John's Hospital for Diseases of the Skin; a month's course in venereal diseases at the London Lock Hospital (if eight entries are received); and eight clinical demonstrations (twice a week) at the School of Tropical Medicine. In the last fortnight of February there will be a course in general medicine and surgery and the special departments at the Prince of Wales's Hospital, Tottenham. In March there will be a course in diseases of the chest, at Brompton Hospital; in medicine, surgery, and gynaecology, at the Royal Waterloo Hospital; in gynaecology, at the Chelsea Hospital for Women; in infectious fevers, at the North-Eastern Hospital, Tottenham; and an intensive course dealing with medicine, surgery, and the specialties, at the Royal Northern Hospital, with which will be associated the Royal Chest Hospital. Copies of the syllabus of the above-mentioned courses may be obtained from the Secretary at No. 1, Wimpole Street, W.1.

THE following courses of lectures will be given at the Ancoats Hospital, Manchester: On Thursdays, February 5th, 12th, and 19th, Mr. Diggle will deal with the diseases of the oesophagus; Dr. Norman Kletz will lecture on dyspepsia on February 26th, March 5th and 12th.

THE Illogan Division of the St. John Ambulance Association has reconstructed its horse-drawn ambulance as a motor ambulance, as a parish memorial to the late Dr. B. J. Mayne.

AT a meeting of the delegates of the Metropolitan Hospital Saturday Fund held on January 21st, the report of the Distribution Committee recommending awards to participating institutions to the amount of £70,488 was adopted; in 1923 the sum of £71,532 was distributed.

DR. J. T. WILSON of Bothwell has been presented by his friends with a silver salver and two fruit dishes in recognition of his services rendered to public health during thirty years of office as medical officer of health for the county of Lanark.

THE late Dr. Henry Gervis has left estate of the gross value of £100,457, with net personalty £37,233. He has bequeathed to the Royal College of Physicians of London a miniature on ivory of Dr. William Hunter by Cosway.

THE *Journal of Scientific Instruments*, a monthly publication dealing with their principles, construction, and use, produced by the Institute of Physics with the co-operation of the National Physical Laboratory, and hitherto published by the Institute, will in future be published by the Cambridge University Press on behalf of the Institute.

THE Dr. Sophie A. Nordhoff-Jung prize of 1,000 dollars for the best work on cancer will be awarded in 1926. Further information can be obtained from the Director of the Department of Biology, Georgetown University, Washington.

DR. E. F. J. PEREGRINE, of Gray's Inn, was called to the Bar on January 26th.

IT is proposed to establish a medical college for women in the outskirts of Tokyo, to accommodate eventually about 700 students.

THE Universities Bureau of the British Empire has issued a report enumerating the students from other countries who were enrolled in the Universities and University Colleges of Great Britain and Ireland, in October, 1924. The total number of students so enrolled was 4,385, of whom 1,573 came from Asia, 1,144 from Africa, 794 from America, 564 from Europe, and 310 from Australia, New Zealand, and Fiji. India, Burma, and Ceylon sent 1,199; South Africa and Rhodesia, 747; the United States of America, 425; Egypt, 309; Australia, 188; Canada and Newfoundland, 162; and New Zealand, 121. Many students from other countries study in England without so enrolling; law students and students in technical schools and research institutes are not included in the figures given above. The report contains also a note of the interchange of teachers in 1923-24 between the Universities of Great Britain and Ireland and those of other countries.

ON January 21st the death took place at Hatch End of Mr. E. J. Sowerby, chairman of Messrs. H. K. Lewis and Co., Ltd. Owing to failing health he retired from active work in April last. He went to Gower Street in 1872, when Mr. H. K. Lewis was still at the head of the business; from 1880 he was manager under Mr. Morris Lewis; in 1905 a partnership was formed of which he was the senior member. In 1915 the present company was created, Mr. Sowerby being chairman, with the other partners as directors. The interment took place on Monday last at Pinner.

Letters, Notes, and Answers.

ORIGINAL ARTICLES and LETTERS *forwarded for publication are understood to be offered to the* BRITISH MEDICAL JOURNAL *alone unless the contrary be stated.* Authors desiring reprints of their articles published in the BRITISH MEDICAL JOURNAL are requested to communicate with the Financial Secretary and Business Manager, 429, Strand, W.C.2, on receipt of proof.

CORRESPONDENTS who wish notice to be taken of their communications should authenticate them with their names—not necessarily for publication.

Communications intended for the current issue should be posted so as to arrive by the first post on Monday or at latest be received not later than Tuesday morning.

THE telephone number of the BRITISH MEDICAL ASSOCIATION and BRITISH MEDICAL JOURNAL is Gerrard 2630 (Internal Exchange). The telegraphic addresses are:

EDITOR of the BRITISH MEDICAL JOURNAL, *Aitiology Westrand, London.*

FINANCIAL SECRETARY AND BUSINESS MANAGER (Advertisements, etc.), *Articulate Westrand, London.*

MEDICAL SECRETARY, *Mediocra Westrand, London.*

The address of the Irish Office of the British Medical Association is 16, South Frederick Street, Dublin (telegrams: *Bacillus, Dublin*; telephone: 4737 Dublin), and of the Scottish Office, 6, Rutland Square, Edinburgh (telegrams: *Associate, Edinburgh*; telephone: 4361 Central).

QUERIES AND ANSWERS.

"R." asks for advice in the treatment of persistent ulcerative Vincent's angina of tonsil.

MOSQUITO BITE.

DR. GERARD C. TAYLOR (Reading), in reply to Dr. Ffennell's query (BRITISH MEDICAL JOURNAL, January 10th, p. 96), writes: In my experience the application of solid menthol is effective in allaying the irritation and minimizing the after-effects of a mosquito bite. It should be rubbed well into the punctures and surrounding skin.

HERPES AND VARICELLA.

DR. C. H. LAVER (Redhill) writes: I should like to record another case of the coincidence of herpes and chicken-pox. On December 25th, 1924, a lady, aged 60, developed herpes of the right ilio-hypogastric nerve. On January 11th, 1925, her daughter, aged 30, who had nursed her mother, developed chicken-pox. This was a well marked case, the vesicles appearing in crops and running their usual course. There was no history of any contact with chicken-pox.

TREATMENT OF TUBERCULOUS MESENTERIC GLANDS.

DR. H. G. FALKNER (London) writes: In answer to "P.'s" question on tuberculous mesenteric glands, I would refer him to *Heliotherapy*, by A. Rollier (London: H. Frowde, Hodder and Stoughton, 1923, price 25s.—reviewed BRITISH MEDICAL JOURNAL, 1923, vol. i, p. 933), and *Artificial Sunlight and its Therapeutic Uses*, by F. H. Humphris (London: H. Milford, Oxford University Press, 1924, price 8s. 6d.). In sun and day light it would seem to be the ultra-violet rays that are of most importance. These rays have a profound influence on metabolism, raising the power of resistance and stabilizing it. This accounts for the many remarkable results got in most tuberculous lesions. I have seen several cases of tuberculous mesenteric glands completely cured by ultra-violet ray treatment.

LETTERS, NOTES, ETC.

HYOSCINE IN PARALYSIS AGITANS.

WITH reference to the note in our issue of December 27th, 1924 (p. 1222), it should have been stated that the hyoscine doses were given twice a day—namely, at 10.30 a.m. and at bedtime—in half a tumbler of water.

FREUDIAN DOCTRINE.

SURGEON REAR-ADMIRAL C. MARSH BEADNELL, R.N., writes: May I record my opinion that the question propounded by Sir Bryan Donkin (December 20th, 1924, p. 1177) as to whether or no a discussion of the Freudian doctrine is desirable and practicable should be answered in the affirmative? But as this particular doctrine is but one among many others believed by the majority of scientific and medical men to be based, not on fact, but on unverified assumptions—all of which constitute a challenging invasion of medical territory—I would extend the scope of inquiry suggested by Sir Bryan Donkin to the formation of a committee of leading medical men and psychologists to investigate the claims of the advocates of these doctrines. A published verdict of such a committee would go far to check what can, in many instances, only be described as exploitation of the unduly credulous. As a whole, the medical profession has, through its aloofness towards such doctrines, failed in its duty to the general public. Three cults—the New Physics, the New Physiology, and the New Psychology—have figured prominently during the last decade, and yet, with certain praiseworthy exceptions, none of these has been seriously questioned by members of the medical profession. The consequence of this deplorable passivity on the part of medical men is that the general public is in the dark as to their views, and in many instances has been grossly misled. To take but one of these pseudo-sciences as a case in point—the New Physics, or, as better known, the Electronic Reactions of Abrams. This alleged diagnostic and curative system, which involves violation of the known laws of physics and physiology, has for long been suspect, yet we have to thank an American committee for showing up the vaunted reactions as non-existent and the treatment as valueless.

The New Physiology or the New Plasmology, as Sir Conan Doyle has dubbed the "science" dealing with the alleged "psychic" exudation called "ectoplasm," has never been seriously investigated by critics in this country. The New Psychology is a term monopolized by Neo-Freudians in lieu of psycho-analysis, but in it must also be included telepathy, or the alleged sympathetic action between two "minds"; telekinesis, or the alleged power to move material objects by "thought" alone; and cryptaesthesia, a faculty of cognition claimed by Richet to be dependent on extrasensorial channels of knowledge. Each of these cults has its famous apostles, yet none of them has been subjected to systematic investigation. It may be said that if a person states that dragons inhabit the centre of the earth the onus of proving their existence is on him and not on his critics, otherwise medical men, psychologists, etc., would be frittering away their lives in disproving the assertions of lunatics and impostors. This argument holds good up to a point, but when credulity is so exploited that widespread harm results to the community, as is now the case, then it is time, I submit, the profession voiced its verdict.

LONG FREEDOM FROM RECURRENCE AFTER OPERATION FOR CANCER OF THE BREAST.

DR. JOHN BROWN (Blackpool) writes with reference to Mr. Steward's case of cancer of the breast in a woman aged 81 years (January 24th, p. 156): I consider the conclusion drawn, that the four cancerous nodules in the scar were connected with the cancer removed thirty-one years before, is fairly open to question. Cancer in persons over 75 is due to degenerative changes in the epithelial cell, and in many cases they are very slow in growth, usually painless, and may be looked upon as a form of natural decay. We know that scar tissue is predisposed to cancerous degeneration. I would call the attention of your readers to an interesting case of chronic cancer of the breast, who died at 97 years of age, recorded by Sir J. Rickman Godlee; there were no enlarged glands. The specimen is in the Royal College of Surgeons of England, Lincoln's Inn Fields, No. 1,774.

AN OPERATION AND AN OBSERVATION.

DR. JOHN B. NASH (Sydney, New South Wales) writes: In the Mackenzie Davidson Lecture by Sir Thomas Horder (BRITISH MEDICAL JOURNAL, July 19th, 1924) he is reported to have said:

> "In reference to operations may I, in passing, share the lament of Sir James Mackenzie that hitherto so little has been done by way of systematic study when the living abdomen is opened and when the thoracic or when the cranial contents are exposed to view—proceedings that are becoming more and more common now that the surgery of the chest and skull is receiving more careful attention? But in the *post-mortem* study of disease, the great element of *function* is missing; and observations during operations upon the living are necessarily very limited so long as the procedures are, as needs must, so rapid, and the surgeon, acting in the patient's best interest, guards so jealously the wound which constantly invites infection."

Several years ago, in conjunction with Drs. Crawley and Dunlop, I attended a medical man in Newcastle. He was desperately ill with double pneumonia and following streptococcal peritonitis. After about three most anxious and arduous weeks for physicians and nurses, the patient began to recover. Several times since then I have heard the local physicians say: "Pituitrin saved the patient's life." Recently I was operating upon a man, aged 58, for a large tumour of the upper third of the neck between the superficial layer of the deep cervical fascia and the prevertebral layer of the same fascia. The necessary procedure included the cutting of the sterno-mastoid muscle in its lower third, the clamping of the carotid artery, the ligaturing of the jugular vein, the separating of the pneumo-gastric nerve from the other contents of the vascular sheath, the lifting of the tumour from its bed on the prevertebral fascia and muscles. The excised mass consisted of the sterno-mastoid muscle, the tumour, the internal jugular vein, the common carotid artery, the external and internal carotid arteries. When this mass was lifted free I drew the attention of my assistants to the regular and forcible pulsation of the tied stump of the common carotid artery, below the omo-hyoid muscle. The anaesthetist said: "He has just been given a dose of surgical pituitrin." The deduction is that the pituitrin dose was just enough to promote the required systoles in the heart muscle, while allowing of the diastoles in regular sequence. The pulsations sent my memory back to the medical man and his desperate illness, and to the pituitrin which made his unstriped intestinal muscles act forcibly enough to empty freely his distended intestinal tract, thereby ensuring his recovery.

VACANCIES.

NOTIFICATIONS of offices vacant in universities, medical colleges, and of vacant resident and other appointments at hospitals, will be found at pages 31, 33, 36, and 37 of our advertisement columns, and advertisements as to partnerships, assistantships, and locumtenencies at pages 34 and 35.

A short summary of vacant posts notified in the advertisement columns appears in the *Supplement* at page 55.